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NOTES AND COMMENTS.

Private Collections.

In March we alluded briefly to the interest of Mr. Du Cane Godman's Presidential Address, recently delivered to the Entomological Society of London. The Address has now been printed, and we make the following extract as worthy of special notice far beyond Entomological circles:—

"Working entomologists, apart from field workers, may be divided, for my present purpose, into two classes, viz., those who are connected with our museums and make the subject their profession and those who are not so employed, but who pursue Entomology at such times as they can spare from their regular business of life. It is the latter, being the more numerous body, who have contributed much more extensively to our present stock of entomological knowledge. Their work has been chiefly based upon their private collections, formed partly from old sources, and partly from new materials often collected by themselves, or brought by field naturalists from all parts of the world. These collections hardly ever have any permanent resting place in private hands; very seldom indeed does a son take up the work of his father; they either change hands, are dispersed, or become—their happiest fate—absorbed in a public museum. This system, which has gradually grown up, is, I think, likely to change, as the extent of collections is, under modern requirements, largely on the increase. This growth of collections renders the subject studied far more complicated, and the difficulty is likely to increase tenfold. The effect is already plainly visible, for private collections are becoming rapidly absorbed by museums, or by a much smaller number of private collectors, with whom they can only temporarily remain. This absorption of private collections by public museums is likely to continue as regards Entomology, as it has in other subjects. It certainly has done so with plants, as now hardly a private herbarium exists in this country—nearly all have

been absorbed by Kew. So also with birds; a large portion of the private collections of a few years ago are now being concentrated in the British and other museums.

"What, then, will be the effect of this concentration of collections in public museums on the number of systematic working entomologists other than the museum officials? Under existing circumstances, I am inclined to think the number will decrease. The necessity at the present time of examining large series of specimens of any species from as wide a range of localities as possible, so that the variation and distribution of the species can be traced, has increased the bulk of collections to an enormous extent. In former times a pair or two of a species was considered enough to represent it in a collection, but now we find that 40 or 50, or even 100 specimens are necessary to show the stability or instability of a species, its range, and all the many points connected with a satisfactory comprehension of its limits. All this vastly increases the cost of forming and preserving new collections, and is against the private collector undertaking the task, and the burden of the work of determining and classifying will be more and more thrown upon the museum officials. That the number of private workers at systematic Entomology should decrease is distinctly to be deplored, and in view of the tendency of collections to become amassed in museums, it follows that it is chiefly to the arrangements made in those museums that this evil is to be arrested.

"As we all know, when once a specimen becomes the property of the Trustees of the British Museum it must never pass out of their possession. Nothing, then, can be seen out of the museum hours, and entomologists who have their daily business to attend to can only visit the museum at rare intervals. They do this to a great extent now in order to determine specimens in their own collections; but when the latter are, from reasons already given, no longer made, the subject will cease to be studied by them. I am inclined to think this can be obviated to a great extent in a way that will greatly benefit the museum and all concerned. There are, no doubt, a considerable number of specimens, such as types, &c., which, on account of their special value, should never leave the museum under any circumstances; they are too valuable to be placed in any risk of loss. Besides these there is by far the greater part of the museum collection which falls into a different category. I refer to specimens which are either unnamed (a very large number), or, if named, are not types, and which in their existing state do not possess any special scientific value, not having had any work bestowed upon them. All such specimens could, at a very slight risk, be entrusted to competent specialists not officially connected with the museum to be named and prepared either for incorporation into the museum series or assigned to the named duplicates for future distribution to other institutions. No doubt additional work would be necessary on the part of the museum officials in preparing such series for examination; but this labour would be more than counterbalanced by the work performed upon the specimens when they are returned named to the museum, and ready for incorporation into the general collection.

"The extent of the subject of Entomology is so vast that nothing but a systematic and continuous effort to amass collections, work them out, and preserve them, can place us in a position to proceed safely with the larger questions which follow the initial step of naming species; and it will only be by the steady effort of our museum

officials, not only to work at the subject themselves, but to enlist the aid of every available outside worker, that real substantial progress can be made. The talents of the outside public can only be fully used by a system by which specimens can be placed in the charge of any competent worker to be determined by him at his own home, and at his leisure, for a certain definite time, and under well understood rules. The benefit to the museum collections under a system of this kind is obvious. The great mass of unnamed materials which we believe now exists in the Museum would be gradually worked into order; and, on the other hand, the inducement of having a good series of specimens to work at, and in a way that suits the circumstances of the outside workers, will certainly tend to increase their numbers, to the great benefit of the study of Entomology."

Mr. Godman speaks with authority as one of the most distinguished of those "unattached" workers on whose behalf he so justly and forcibly pleads. We cordially endorse his sentiments from the point of view of many other branches of Natural Science, to which his remarks are equally applicable. The rules of the British Museum have the sanction of antiquity, have worked well on the whole thus far, and are scarcely likely to be changed; and this notwithstanding the fact that our National Museum is unique in the world, so far as we are aware, in denying its custodians all powers of discretion in dealing with applications for the loan of materials. We confess, however, to a more sanguine view than that of Mr. Godman on the subject of the private collections of the future. No officialism can entirely extinguish the independent worker, whose mainspring is his enthusiasm, and whose base of operations lies in his collecting. We believe the tendency of modern research is merely to limit the scope of such collecting, and we hope and trust that as progress demands further attention to details the aims and methods of collectors will be modified accordingly. By far the largest proportion of our national collection is due to the individual efforts of independent non-professional naturalists contributing their several quota.

The Exploration of Matto Grosso, Brazil.

We have been favoured by Dr. J. W. Evans and Mr. Spencer Moore with some brief notes on their recent expedition to the remote Brazilian province of Matto Grosso, to which we referred last month. The exploration was undertaken under the auspices of a syndicate interested in the reputed mineral wealth of Matto Grosso, and its partially scientific character is believed to have been due to the influence of the late Emperor, Dom Pedro II. Messrs. Evans and Moore proceeded *viâ* Buenos Ayres and the River Parana to Cuyabá, the capital of Matto Grosso, and under the able guidance of Mr. Whitehorn, an English gentleman settled in Paraguay, they at once started for the Chapada plateau, the escarpment of which, situated some miles to the east of Cuyabá, is estimated to reach the height of over 2,000 feet above sea-level. This plateau owes its existence to the

superposition of sandstones upon slates more easily affected by denudation; the former rock, from its containing a few obscure Brachiopod fossils, discovered by Mr. White, a naturalist who spent some time in the district, is considered by Dr. Derby to be of Devonian age. The country between Cuyabá and the plateau is of the open kind known as *monte*, various species of *Ochna*, *Erythroxylon*, a *Cochlospermum* in August leafless and one mass of golden bloom, the Vochysiaceous tree *Salvertia convallarioides*, Bignonias, Bromeliaceæ, &c., being conspicuous objects in its vegetation. A stay of two days was made at the village—now much decayed—of Sta. Anna da Chapada, after which the party returned by easy stages, securing specimens of the rocks and Natural History objects by the way.

A visit was subsequently paid to Santa Cruz, followed by a journey in a north-west direction through the forest whence the province derives its name, and where is found a great part of the Ipecacuanha which reaches Europe. After considerable difficulty the forest was traversed, and the party emerged upon the fine treeless Campos de Tapirapuan, which are flanked by densely wooded hills (Sierra de Tapirapuan) running east and west. The existence of an olivine basalt in these hills was the most interesting discovery made hereabouts. Returning to Santa Cruz, the party devoted ten weeks to the examination of that district, and the naturalists made a large collection of the rocks, plants, and animals, while Mr. John Storm devoted himself to the correction of the faulty maps of this part of Brazil.

Notwithstanding the early and unexpected recall of the expedition, we are glad to learn that some results of much scientific interest may be expected from the study of the collections brought to Europe.

Ants and Mites.

The curious habit which ants have of harbouring in their nests a variety of other insects, is a well-known fact. The reason for this singular exercise of hospitality is by no means always apparent; in some cases, however, it does appear to be fairly clear, particularly in the case of certain mites (Gamasids), whose habits and customs are treated of by Mr. A. D. Michael in the recently published part of the *Proceedings* of the Zoological Society of London. The author of this paper investigated a number of ants' nests in Corsica and in the neighbourhood of Innsbrück, and in many of these nests there occurred various species of Gamasids, whose relation to their host formed the subject of the enquiry dealt with in the paper. The nests of a small yellow ant, *Tetramorium cæspitosum* var. *meridionale*, were infested with two kinds of Gamasids. One species, which Mr. Michael describes as new, under the name of *Lælaps equitans*, was not only found in the nests, but also upon the ants themselves; and

when the nests were disturbed the mites, being slow of foot, leaped on to the head of a passing ant, and were borne off to a place of safety. The ants appeared to have not the slightest objection to this familiarity on the part of their guests; on the contrary, indeed, for they carried off the mites without making the least attempt to dislodge their riders, and ants are not as a rule the most peacefully disposed of animals. In the case of another species of ant, the care taken of the Gamasids was even more remarkable. When danger threatened the colony, the ants carried off both the mites and their young, just as they carry off their own young. After a careful series of experiments, Mr. Michael comes to the conclusion that the mites repay the hospitality shown to them by removing the bodies of deceased ants, which they utilise as food.

South American Extinct Mammalia.

In a recent issue of the *Anales d. Mus. Nat., Buenos Aires*,[1] Dr. Hermann Burmeister continues his important researches on the fossil mammals of Argentina. Dr. Burmeister is one of those designated under the somewhat inelegant title of "lumpers"; and, although he may perhaps err somewhat in the opposite direction, he is certainly doing good work in calling attention to the reckless manner in which so-called genera and species have been named by some of his fellow-workers. Indeed, when, according to their own showing, no less than five genera, to say nothing of species, have been manufactured by the latter out of remains belonging to *Nesodon imbricatus* and *N. ovinus*, while Dr. Burmeister adds three more to the list, it is high time that something were done to check this endless flow of names—names in many cases no sooner published by one writer than they are relegated to the rank of synonyms by another. In addition to showing the absolute invalidity of some of these names by actual comparison of the specimens on which they were founded, our author in other cases shows that for physical reasons it would be an absolute impossibility for such a number of gigantic animals as have been named to exist within the limits of the country.

A large portion of the memoir is devoted to a description of the Ungulate genus *Nesodon*, a near ally of the huge *Toxodon*; but we have also notices of other interesting forms. Among these is the occurrence of a species of the Edentate genus *Megalonyx* (as typified by *M. jeffersoni*, of Kentucky) in the Tertiaries of South America, which is a point of considerable interest in distribution. Not less important from another aspect is Dr. Burmeister's statement that the alleged occurrence of enamel in the teeth of certain of the Ground Sloths of the Lower Tertiaries of Argentina is due to osteo-dentine having been mistaken for enamel. By this we have at a single stroke the genera *Promegatherium* and *Promylodon* severally united with *Megatherium* and

[1] Vol. iii., pp. 401–488, pls. viii.–x. (1891).

Mylodon; and the range of the two latter is consequently carried much further back in the Tertiary period.

The remaining portions of the work are mainly devoted to criticisms of genera and species founded by writers in Europe, the majority of which are regarded as invalid.

It is, perhaps, too much to expect that Dr. Burmeister's caustic criticisms will at once check the reckless multiplication of names now going on in South America; but it will at least have the effect of drawing attention in Europe to the serious nature of the evil. Palæontologists in Argentina have a magnificent field before them, but it is not too much to say that some of them have so involved the subject in a wilderness of meaningless terms, that many European workers are repelled from giving to the fossil mammals of Argentina the attention they so undoubtedly merit.

REFERRING to the manner in which extinct mammals are named in the New World, we may direct attention to the following paragraph from a paper by Professor Marsh in the April number of the *American Journal of Science*, p. 351:—

"The oldest ancestor of the horse, as yet undiscovered, undoubtedly had five toes on each foot, and probably was not larger than a rabbit, perhaps much smaller. This hypothetical predecessor of the horse can now be predicated with certainty from what is known of the early hoofed mammals. It may be called *Hippops*, and its remains will be found at the base of the Tertiary, or more likely in the latest Cretaceous."

We have known cases of the description of genera and species by telegraph, and we have unfortunately had some experience of the ordinary struggle for "priority" in nomenclature; but we have never yet met with anything equal to this latest development. Who, we should like to know, is to decide which of the numerous small five-toed hoofed animals that will probably be discovered someday at the base of the Tertiaries must bear the name of *Hippops?*

AN important memoir on the Oligocene mammals of the so-called Siderolites of Egerkingen, near Soleure, Switzerland, is contributed by Professor L. Rütimeyer, of Basle, to the publications of the Swiss Palæontological Society.[1] It is largely owing to the previous researches of the Professor that our knowledge of this interesting and prolific fauna is due; and the present memoir affords much additional information. The illustrations are excellent, and ought to aid materially in comparisons with specimens from other localities. Several forms previously known only from the nearly contem-

[1] "Die Eocäne Säugethier-velt von Egerkingen," *Abh. schweiz. palæont. Ges.*, vol. xviii., pp. 1–153, pls. i–viii. (1892).

poraneous Phosphorites of Central France, are now identified from the Siderolites; while of still wider interest is the identification of certain Swiss forms—especially Lemuroids—with genera from the Eocene of the United States. We note that the Professor does not accept the identification of the Lemuroid from the French Phosphorites described as *Necrolemur*, with *Microchœrus* of the English Oligocene; and it seems probable that some emendation will have to be made in regard to the mammals he refers to the genera *Hyracotherium* and *Hyopotamus*. Some interesting observations are recorded as to the homology and nomenclature of the component elements of the molar teeth; and the Egerkingen fauna is carefully correlated with other faunas of the same age. We must venture to take objection to the title page of the memoir bearing the date of 1891, when it is perfectly clear from internal evidence that it was not published until the present year was far advanced.

In the February number of the *American Naturalist* just received, Professor Cope discusses the affinities of the so-called "Marsupial Mole" (*Notoryctes typhlops*) from Australia, lately noticed in Natural Science (p. 36). He considers that its relationship to the Marsupials is not proved, and suggests that it ought rather to be placed with the Insectivora. He especially emphasises the resemblance of *Notoryctes* to the South African *Chrysochloris*, and thinks that the Notoryctidæ may belong to one of the groups required to connect the marsupial and placental mammals. He adds that the existence of a South African type of placental mammal in Australia is not surprising, since the freshwater fish *Gonorhynchus greyi* is common to both countries, and the ratite birds and pleurodire tortoises are found in both.

The subject of the so-called "tailless" trout met with in several of the Scottish lakes is discussed again by Dr. R. H. Traquair in the April number of the *Scottish Annals of Natural History*. The tail-fin in these trout, instead of being forked and fan-shaped, is rounded and degenerate. Many new facts are added, and there are several beautiful figures; but the explanation of the strange phenomenon is still as mysterious as ever.

Captain Chaves, of the Portuguese Army, who has made a special study of the fauna of the Azores, has lately found a very remarkable crustacean off the Isle of San Miguel. It is a Mysid of remarkable appearance, covered with an overhanging carapace of very large size (8 or 9 millim.) compared with the total length of the animal (12 millims., not including antennæ). Under this carapace, which is armed with spiny processes and characteristic ridges, the

limbs, abdomen, and tail of the animal are completely covered when at rest. The latter peculiarity suggested the name of *Cryptopus*, given to these aberrant Mysids by Latreille in 1829. They do not appear to have been seen again for more than fifty years. M. Chaves discovered them in the stomach of a dolphin-fish (*Coryphæna equisetis*). There are several specimens of the same species (*Cryptopus defrancii*) in the British Museum, and Dr. J. E. Gray described them in 1830 as the type of his genus *Cerataspis*; these fragments were obtained from the stomach of a dolphin off the coast of Brazil.

THE discovery of an elasipodous Holothurian in the Southern Mediterranean has just been announced by Dr. E. von Marenzeller, before the Imperial Academy of Sciences of Vienna. It is a colourless species, scarcely 20 millims. in length, belonging to the family of Elpidiidæ, and apparently approaching the genus *Kolga*. Numerous examples have been taken in three localities, at depths of 798, 943, and 1,292 metres, but none are well preserved. The discovery is of great interest, for the known Elasipoda are restricted almost exclusively to the cold polar seas and the depths of the ocean: a single exception to the rule is mentioned in the "Challenger" Report, vol. iv., where *Uyodæmon maculatus* is recorded from the Philippines in water at a temperature of 21° C.

IN the last part of the *Quarterly Journal of Microscopical Science*, Mr. Beddard describes the structure of a new genus of Oligochæte worm, named *Branchiura* on account of its nearly unique peculiarity of possessing gills. The worm was found in the "Victoria Regia Tank" of the Botanical Society's Gardens, in company with another very remarkable worm, lately recorded from Madras by Professor A. G. Bourne. The latter, which is a Naid, has also been quite recently described in the same journal; it possesses outgrowths in the anterior region of the body which enclose the bristles or setæ, and are probably gills in function, though they appear to suggest the parapodia of the marine Chætopods. In *Branchiura* the gills are limited to the hinder segments of the body, and are dorsal and ventral in position. There is a pair to each segment; they are contractile and recall the cirri of certain marine worms. The only other Oligochæte known, besides the two just mentioned, which agrees with the Polychæts in having gills, is *Digitibranchus niloticus*, recorded a great many years ago from the Nile by the late Professor Grube. Unfortunately the description of this creature is rendered nearly useless by the absence of any account of its structure; it is impossible to be certain that it is not after all a Polychæte. *Branchiura* belongs to the family Tubificidæ, typified in this country by the common slender red worm, *Tubifex rivulorum*, so abundant in rivers, ditches, and ponds.

We understand that Mr. W. H. Hudleston, F.R.S., has in the press a "Catalogue of the Gasteropoda of the Inferior Oolite Formation." It will be modelled on Morris's "Catalogue of British Fossils," published in 1854. The mention of Morris's Catalogue reminds us of a Committee that was formed at the author's death to perpetuate his memory by publishing a new edition of this well-known work. The labours of that Committee will be considerably reduced if they remain in obscurity much longer, for the vertebrata were some time ago taken off their hands, and soon they will be relieved of part of the invertebrata.

A "Sketch History of Marlborough in Neolithic Times" (read before the Toynbee Hall Natural History Society, on their visit in 1891) has just been issued by Mr. F. J. Bennett. Referring to the lynchets or cultivation-terraces met with on the White Horse Hill, also near Collingbourn and Lambourn, he expresses his agreement with Mr. Gomme that they were formed independently of the plough, and, indeed, before its invention. He cites instances where the lynchets must have been cut out of the hill sides, the retaining-walls having been faced with flints or sarsen stones.

Notes on the rock-fragments that occur in the Permian Breccia of Leicestershire are communicated by Professor T. G. Bonney to the *Midland Naturalist* for February and March. The specimens, which were collected by Mr. W. S. Gresley, include, as might be expected, many of the Charnwood rocks, but in addition to the fragments from these old and possibly pre-Cambrian formations, there are some from the Carboniferous Limestone and (probably) Millstone Grit, and many from the Coal-measures. The evidence afforded by these rock-fragments of unconformity between the Coal-measures and Permian strata, confirms and supplements that obtained by Mr. Horace T. Brown and published by the Geological Society in 1889. Certain striated fragments have been found in the Breccia. These, in Professor Bonney's opinion, might in some cases have been scored by other rock-fragments; but in no instance is he convinced that ice-action had any connection with the phenomena. Nevertheless, referring to the general characters of the accumulation and to certain boulders that have been found in the Permian beds at Coton Park, he admits that if we are unconvinced by the striæ, the general evidence of the breccias of this Leicestershire district seems favourable to the idea that a rather low temperature prevailed in Permian times.

The subject of Mean or Average Rainfall was brought before the Institute of Civil Engineers on March 22 by Mr. A. R. Binnie.

He came to the conclusion that the laws which govern the fluctuations are, apparently, of general application, and must be sought for in some cause, yet unknown, but yet common to the whole world. The facts he had tabulated showed that when the mean rainfall exceeded 20 inches, the geographical position of stations had little to do with the variations.

THE first number of a new botanical work, *Phycological Memoirs*, edited by Mr. George Murray, has just been issued by Messrs. Dulau & Co. The object of the publication is to keep "within the limits of one book the results of researches on Algæ in the Department of Botany of the British Museum." Further parts are promised at about half-yearly intervals, and will in time complete a volume containing about 50 plates. "In addition to the promotion of research, the authors of the memoirs have the object of making better known the treasures of the Museum which they have received facilities to investigate." The new venture promises well, judging from Part I. In the first paper, Miss Mitchell and Miss Whitting describe the type of a new Natural Order of Algæ, allied to the Fucaceæ and Laminariaceæ. Next follows the description of a fossil alga from the Oolite, by Mr. Murray. The third paper, also by Mr. Murray, deals with the structure of *Dictyosphæria;* and there are important contributions by Miss Barton and Mr. E. A. Batters.

I.

Dr. Weismann's Theory of Heredity applied to Plants.

THE essential point of Dr. Weismann's theory of reproduction is the assumption of a special reproductive substance or *germ-plasm*, distinct from the rest of the protoplasm, which is *somatic*, serving to nourish the germ-plasm and maintain the growth of the individual. There is a continuity of this reproductive substance from one generation to another, each passing on to its offspring a certain amount of germ-plasm which, nourished by the somatoplasm, is increased in the individual but undergoes no change in kind, so long as it simply perpetuates the species.

Nägeli distinguished two parts in the protoplasm; an active organising portion termed *idioplasm* being separated from the rest which is merely nutritive. But while Nägeli's idioplasm is regarded as forming a continuous network throughout the organism formed by growth from the idioplasm of the spore, Weismann's germ-plasm is confined only to a certain series in the *soma* and a few special cells of the individual, which are accordingly distinguished as reproductive from all the rest which are merely somatic.

In the following observations on Dr. Weismann's theory,[1] I propose limiting my remarks to the consideration of three points only, as follows:—

1. That there is a continuity of germ-plasm, from the germ-cells of the parent to those of the offspring (p. 173) in a certain series of somatic cells only (pp. 197, 207).

2. That the characters acquired by the individual through "use, disuse, practice, neglect, nutrition, light, moisture, and climate," (p. 387) cannot produce any effect in the transformation of species (p. 388) and cannot be transmitted at all (p. 267).

3. That the transformation of species is due (i.) to direct changes in the germ-plasm effected by external influences (pp. 327, 388, 396,

[1] *Biological Memoirs*, Authorised translation, Oxford. The pages given in the text refer to the First Edition, 1889.

411); or (ii.) by combinations of distinct germ-plasms by sexuality (pp. 293, 321, 327).[2]

1. The Continuity of Germ-plasm.

The following quotations will express Dr. Weismann's views on this point in his own words:—"A continuity of the germ-*cells* does not now take place, except in very rare instances; but this fact does not prevent us from adopting a theory of the continuity of germ-*plasm*" (p. 173). "That continuity is maintained, by assuming that a small part of the germ-plasm persists unchanged during the division of the segmentation-nucleus, and remains mixed with the idioplasm of a certain series of cells, and that the formation of true germ-cells is brought about at a certain point in the series by the appearance of cells in which the germ-plasm becomes predominant" (p.197). This "point" is, on the one hand, the nucleus or egg-cell of the embryo-sac, and, on the other, the sperm-cell or fertilising nucleus of the pollen-grain (p. 402); for "the nuclear substance must be the sole bearer of hereditary tendencies" (p. 179).

With regard to the mode of development of hereditary characters, whether in the vegetative or reproductive organs of plants, he quotes Strasburger, who says, "the nuclei determine the specific direction in which an organism develops" (p. 185). "The nucleoplasm will enter upon definite and varied changes which must cause the differences appearing in the cells which are produced" (p. 185). "The complexity of the molecular structure [of nucleoplasm] decreases as the potentiality for further development also decreases" (p. 191); and therefore, "it has lost the power of originating any new kind of cell" (p. 195). Therefore, "A sudden transformation of the nucleoplasm of a somatic cell into that of a germ-cell would be almost as incredible as the transformation of a mammal into an amœba" (p. 200). "There is no nucleoplasm like that of the germ-cell in any of the somatic cells" (p. 195), excepting presumably the germ-plasm contained in "the certain series of cells" (p. 197) by which its continuity is maintained.

The above deductions were drawn from Dr. Weismann's study of the Hydroids, on which he thus speaks:—"As a result of my investigations on Hydroids, I concluded that the germ-plasm is present in a very finely divided, and therefore invisible state in certain somatic cells, from the very beginning of embryonic development, and that it is transmitted through innumerable cell-generations to those remote individuals of the colony in which sexual products are

[2] Dr. Weismann has also said elsewhere:—"A species is only caused to change through the influence of changing external conditions of life, the change being in a fixed direction. . . . Transmutation by purely internal causes is not to be entertained. . . . The action of external inciting causes in the widest sense of the word is alone able to produce modifications." *Nature*, vol. 22, p. 141.

formed. This conclusion is based upon the fact that germ-cells only occur in certain localised areas" (p. 207).

Dr. Weismann now endeavours to apply his hypothesis of germ-plasm being limited to a certain series of somatic cells to plants. Since, however, Begonias can be propagated by their leaves, he admits:—"I think that this fact only proves that in *Begonia* and similar plants, all the cells of the leaves, or, perhaps, only certain cells, contain a small amount of germ-plasm, and that consequently these plants are specially adapted for propagation by leaves" (p. 211). He then proceeds to base an argument upon this, as follows:—"How is it then that all plants cannot be reproduced in this way? No one has ever grown a tree from the leaf of the lime or oak, or a flowering plant from the leaf of the tulip or convolvulus." The author here infers that because no one has raised tulips from their leaves, therefore it cannot be done! The fact, of course, really is that it has never been worth while to do it. If, however, a premium were offered for tulips or oak trees raised from leaf-cuttings, plenty would soon be forthcoming. It is known to all horticulturists and botanists that there is virtually no limit to the propagation by leaves or any other part of a plant.[3] The sole conditions are the presence of "embryonic" cells, and a sufficient amount of nutritive material at the section to enable the part to develop roots.

Reading in Sachs' lectures on the Physiology of Plants,[4] that in the true mosses almost any cell of the roots, leaves, and shoot-axes, and even of the immature sporogonium, may grow out under favourable conditions, become rooted, form new shoots, and give rise to an independent living plant, Dr. Weismann makes the further admission: "We have here a case which requires the assumption that all or nearly all cells must contain germ-plasm" (p. 212). He does not seem aware that the propagation by roots, stems, leaves is habitually practised in *all* kinds of plants whenever desirable, that the power of hereditary reproduction by the vegetative system is no special peculiarity of Begonias and Mosses, but a universal feature of the vegetable kingdom. Even fruits such as cucumbers, prickly pears, and strawberries have been known to send out roots and buds.

This fact is, of course, perfectly well-known to all botanists and horticulturists, and needs no further illustrations. Dr. Weismann's mistake lies, therefore, in supposing the phenomenon to be exceptional instead of general.

Moreover, besides the production of flowers, fruits, and seeds from the vegetative system, it must be remembered that roots, usually confined to subterranean regions, can be produced anywhere in the

[3] For example, though it is usual to propagate Chrysanthemums by stem-cuttings, a florist exhibited at one of the exhibitions of the Royal Horticultural Society a large quantity raised from leaves. The Dutch propagate Hyacinths solely rom sections of leaf-scales, &c.

[4] P. 723, English Edition.

aërial parts of plants. Similarly, the tubers of the potato, a specific character, as the name *Solanum tuberosum* implies, can be made to appear on the upper part of the plant, by not allowing them to grow underground. Lilies will sometimes develop bulbils on their stems abnormally, just as *Lilium bulbiferum* normally produces them. All such, and many other cases that might be mentioned, appear to indicate that if germ-plasm be requisite for the development of hereditary characters, it must be present practically everywhere, and in readiness to produce them when called upon to respond to external influences which give rise to them.

Hence we arrive at the necessity of extending Dr. Weismann's hypothesis and, adopting his own words, to say: "That *all* somatic nuclei of plants may contain a small amount of unchanged germ-plasm" (p. 211).

2. Acquired Characters are not Hereditary.

Dr. Weismann defines them as being "those characters which have been acquired by the influence of special external conditions during the life-time of the parent; such cannot be transmitted at all" (p. 267). "They cannot produce any effect in the transformation of the species, simply because they can never reach the germ-cells from which the succeeding generation arises" (p. 388). Acquired characters he considers to be such as are attributable to "use and disuse, practice or neglect, nutrition, light, moisture, and climate" (pp. 387, 8). On the other hand, he asserts that "changes in the germ-plasm are the first results of the new conditions" (p. 414).

Thus, in the case of wild carrots cultivated by Hoffman, in which changes produced by cultivation in the structure of the root were hereditary, Dr. Weismann observes:—"It is sufficiently obvious that in this case we are also concerned with a change which did not become visible until after some generations had elapsed, and which was therefore a change in the germ-plasm" (p. 414).

It would seem not unreasonable to demur to his supposed necessity of the effect being visible at first, for it is quite conceivable, and indeed probable, that time may be required in most cases for the influences of the environment to accumulate in order to produce any marked effects. Moreover, although no changes may be externally visible for two or more generations, yet the effects may be recorded in some alterations in the tissues only observable by the microscope, and are potentially accumulating until at last they become very manifest to the naked eye. Indeed, no one, probably, has as yet actually examined the roots microscopically with the express purpose of testing this point, for the attention of botanists has not been brought to it before.

In the case, however, of certain experiments conducted by the late Professor J. Buckman with parsnips and carrots, the change *was*

actually produced and visible in the first season. He thus describes his results:—"In the spring of 1848 I planted two of my experimental plots[5] with seeds of *Pastinaca sativa* and *Daucus carota*. The seeds were gathered in the previous summer from wild plants. In the autumn, whilst most of the specimens presented the dark green hairy leaves peculiar to the wild plant, there were a few examples in which the foliage had assumed the light green and smooth aspect devoid of hairs which characterises the cultivated plant, and among the latter there were a few with larger leaves and broader divisions, or leaf-lobes, than the rest, the leaves, too, all growing symmetrically around one central bud. The roots of the plants taken up were observed to be, for the most part, more fleshy than those of the wild examples."

In the spring of 1849, the reserved roots were dug up, and the best put aside for transplanting. Late in the summer of 1849, the seed was collected, and sown in 1850.

The result of the second sowing was that the leaves in all indicated an advance to the cultivated form. Some specimens were much finer than others, of good size, and with a tolerably smooth outline.[6]

"In the case of carrots of the first crop all the specimens manifested a disposition to send up several heads, and most of them ran to seed. The roots were much forked, and but little inclined to increase the tissue. The second crop was certainly improved, the crowns of many displayed but one bud. The roots became more succulent and enlarged."[7]

This experiment seems to justify my remarks, for it is evident that *all* the plants, both of parsnips and carrots, were not equally affected by the new environment, and while in some the changes were "visible" at once, in others, especially the carrots, it required a generation to evince them satisfactorily. The probability is—judging from the pronounced way in which *some* were modified—that all were more or less influenced, and had Professor Buckman examined the tissues of the roots he might possibly have detected the commencement of hypertrophy in the first season in every one.

As another instance of experimental evidence, I will refer to that of M. Lesage upon maritime plants. It is usual to attribute the fleshy character of the leaves of several maritime plants to their growth by the seaside, where salt can reach them; and that salt is

[5] In the garden of the Agricultural College, Cirencester, at which he was the Professor of Natural History.

[6] Professor Buckman called one "The Student," a hollow-crowned form, and forwarded seeds to Messrs. Sutton & Sons to grow; they approved of it, and issued it as "Improved," in 1851. On writing to them in 1890, they informed me that they still consider it the "*best variety*" in their stock. Hence it has been now established in the trade for 40 years. In 1891 they issued a new illustration of "The Student," which shows marked differences from that drawn in 1861.

[7] *Journ. Agricult. Soc., England*, 1864.

the *cause* of the fleshiness, is believed, from a large number of coincidences, coupled with the fact that the same plants are not always fleshy when growing inland. M. Lesage examined the tissues of a large number of plants growing both near and away from the sea; and he came to the following conclusion:—"Il résulte clairement du rapprochement de ces faits que le voisinage du bord de la mer en même temps qu'il détermine l'épaississement de la feuille, y provoque le développement exagéré du tissu palissadique."[8]

Then, following up the investigation experimentally, he succeeded in producing an identically similar fleshiness by watering with salt water, thereby verifying the deduction that it was due to the presence of salt from the sea. He adds:—"J'ai réussi sourtout pour le *Lepidium sativum* cultivé en 1888. J'ai obtenu pour le même plante, des résultats plus nets encore dans la culture de 1889, entreprise en semant les graines récoltées avec soin dans les pots de l'année précédente et traitées exactement de le même façon."[9]

This experiment, like Professor Buckman's, shows that the alteration of the tissues was carried on in the second generation from the point gained in the first. In both cases no germ-plasm (in the germ-cells) existed at the time during which the alterations arose, as they were confined to the vegetative system; and in the case of the parsnips and carrots, being biennials, no germ-cells are produced till the second year has arrived.

As a third example, Professor Bailey observes:—"Squashes often show remarkable differences when grown upon different soils, and these differences can sometimes be perpetuated for a time by seeds. The writer has produced from the same parent squashes so dissimilar, through the simple agency of a change of soil, in one season, that they might readily be taken for distinct varieties. Peas are known to vary in the same manner. The ends of a row of peas sown of the same kind, last year gave the writer marked variations, due to differences in soil. . . . Pea-growers characterise soils as 'good' and 'viney.' Upon the latter sort the plants run to vine at the expense of the fruit, and their offspring for two or three generations have the same tendency."[10]

Once more. It has often been observed that spinescent features are characteristic of arid regions. M. Lothelier[11] found that the Barberry produced only spinescent leaves in a dry air, and none at all in a moist one. I find by experiment that *Ononis arvensis* loses its spines in a rich, moist soil, whether grown from cuttings or raised from seed.

As a further illustration in which climatic influences appear to

[8] *Rev. Gén. Bot.*, vol. ii., p. 64. [9] *Loc. cit.*, p. 114.

[10] Extracts from an article in the "American Garden," by Professor Bailey; quoted in the *Gardeners' Chronicle*, May 31, 1890, p. 677.

[11] *Rev. Gén. Bot.*, 1890, p. 276.

be the direct cause, is the fact that the species of cultivated plants often, if not generally, "sport" simultaneously in various districts, and even in different countries. Thus, I am assured that double Petunias were never raised until they simultaneously appeared in one and the same season in France, Germany, and England. Similarly, Chrysanthemums are known to furnish sports of a like kind in different places. Now, although double flowers, as of Petunias, are mostly propagated from cuttings, yet, speaking generally, they can be perpetuated by seed and intensified by self-fertilisation. It was thus that Mr. Veitch raised his "Balsamifloral" section of East Indian Rhododendrons, and others raised double Begonias.

Again, there is a class of phenomena which are designated as "habits," as of insectivorous, parasitic, saprophytic, and climbing plants, &c. These are scattered over the vegetable kingdom, often without any affinities, respectively; and when one imagines the *first individual* of any species which acquired any one of these habits, becoming, *e.g.*, parasitic, such a habit must have been a character acquired during the lifetime of the individual. Yet, in all cases these habits, and the anatomical structures correlated to them, are now hereditary.

As other instances in which peculiar structures are now hereditary may be mentioned aquatic plants, and those possessing subterranean stems. Whether they be dicotyledons or monocotyledons, there is a fundamental agreement in the anatomy of the roots and stems of aquatic plants, and, indeed, in many cases, in the leaves as well. Such has hitherto been attributed to the aquatic habit. The inference or deduction was, of course, based upon innumerable coincidences; the water being supposed to be the direct cause of the degenerate structures, which are hereditary and characteristic of such plants in the wild state.

M. Costantin has, however, verified this deduction by making terrestrial and aërial stems to grow underground and in water. One could not say, *à priori*, what alteration would take place, if any at all; but the new structures at once begin to assume the subterranean or aquatic types, as the case may be; and, conversely, aquatic plants made to grow on land assume the terrestrial type of structure; analogous results following changes from a subterranean to an aërial position, and *vice versâ*. This verification, therefore, proves not only that the influence of the environment is at once felt by the organ, but that it was indubitably *the cause* of the now specific and hereditary traits peculiar to normally aquatic, subterranean, and aërial stems or roots.

The difference in structure between the upper and under side of a horizontal leaf is known to be interchangeable, according to the incidence of light; while erect leaves, equally exposed, are similarly constructed on both sides. Here, as in all organs, we discover by

experiment how easily the anatomy of plants can be affected by their environment; and as long as the latter is constant, so are the characters of the plant constant and hereditary.

Dr. Weismann, however, would attribute such susceptibilities to an assumed "predisposition," instead of regarding it as a universal property of protoplasm to respond to external influences. One cannot disprove the existence of such a predisposition, but it seems unnecessary to assume it, when probabilities are accumulative on the other side, which render it entirely unnecessary.

As an instance of a natural change of structure and habit, I have often found *Ranunculus heterophyllus* growing on mud after the water has dried up. As in the case of the normally submerged plant, finely dissected foliage had still grown, but altered in character. The segments had become rigid, broader, and with stomata. The leaves were all erect, forming a sort of green "pile," and perfectly adapted to aërial conditions. Such may, perhaps, be compared to the Mud-fish, which can breathe in both water and air. In another case the plants became prostrate, rooted at the joints, formed no more dissected leaves, and were in all respects similar to a form of *R. hederaceus*.

Similar alterations occur in terrestrial plants raised by seed sown in pots, collected from the aquatic form. The cotyledons at once appear of a different character. Then, the first leaves are dissected, but "aërial," the subsequent ones being of the "floating" type; as if the plant had reached the surface of some imaginary water! This illustrates, therefore, very well what is a universal feature, viz., the combination of hereditary characters in conjunction with direct adaptations to a new environment. If the above plants be transferred to water, the "aërial" dissected leaves die, and "aquatic" dissected leaves are immediately developed.

As Dr. Weismann includes "climate" as one of the forces acting on the individual which may produce "acquired characters," I may add the effects produced by a change from an Alpine to a low-level habitat, as well as from a high to a low latitude. It is well-known that there are not only plainly visible differences, such as a richer foliage and brighter coloured flowers, in high latitudes and Alpine regions, but the anatomical details of structure of the leaves are very different.

As in the case of experimental evidence of the effects of water, so in the case of these conditions, M. Bonnier[12] has proved by cultivation that when plants are changed from one locality to another, being grown from seed, the vegetable system soon begins to respond to the new climatic conditions, and assumes the histological characters of the plants normally growing in the same environment.

[12] *Bull. Soc. Bot. France*, 1886, p. 467. See also *Ann. des Sci. Nat. Bot.*, 1879, and *Rev. Gén. Bot.*, vol. ii., 1891, p. 513.

The evidence of the direct influence of the environment as causing changes of structure is thus accumulative. Indeed, what is surprising is not so much that changes are produced, as the constancy of species in Nature; for any pronounced alteration of habitat seems almost always to set up alterations of structure. Such is the experience of horticulturists, for when, for example, bulbous plants, as tulips, &c., are first introduced from the East into English gardens, they soon begin to change in all parts of their structure, so that in a few years they are hardly recognisable.[13]

3. Transformation due to direct Changes in the Germ-plasm.

In maintaining the proposition that all hereditary structures have arisen through changes first wrought in the germ-cells alone, and must have been produced through the influences of external agencies working directly upon the germ-plasm, Dr. Weismann seems to have raised at least two very formidable and unnecessary difficulties so far as plants are concerned. Firstly, the growth and development of all vegetative structures takes place, and is mostly completed, *before* any germ-cells are produced, the flowering process (which alone contains them) invariably following the close of the period of vegetative growth. Secondly, while the duration of the vegetative period is relatively long, that of the existence of the germ-cells is excessively short. Thus, no flowers last for more than a few hours only. On the other hand, an annual requires at least several weeks for its development; perennial herbs and trees demand months and years; an aloe may be fifty years old before it blossoms, while ferns and horsetails never have any germ-cells at all during the whole of the agamic generation.

If, however, we suppose with Strasburger that germ-plasm be everywhere present and capable of being influenced by the environment, then these difficulties are at once removed, and the probability is much increased that variations have come about through the responsiveness of germ-plasm (assumed to accompany the protoplasm) coupled with the power of fixing the effects and of transmitting them to the germ-cells on the appearance of the latter at the period of flowering.

Dr. Weismann's objection that "acquired characters cannot be transmitted at all" (p. 267), because "they can never reach the germ-cells" (p. 388), is thus met. For if germ-plasm be always "unchanged," and capable of receiving and registering impressions from without, wherever it may be, and not solely within the germ-cells, as he seems to maintain, we thus have at least a sufficient material.

Here, again, we have admissions from him that this is con-

[13] Such, *e g.*, is the experience of Mr. Elwes. *See also* "Sur la production et la fixation des Variétés dans les plantes d'ornement," by B. Verlot, 1865.

ceivable, for he says:—"It must be admitted that there are cases, such as the climatic varieties of certain butterflies, which raise some difficulties against this explanation. I myself experimentally investigated one such case, and even now I cannot explain the facts otherwise than by supposing the passive acquisition of characters produced by the direct influence of climate" (p. 99).

The next and last point is the *machinery* required for transmitting the effects of the environment, presumably produced at the periphery of the organism, to the germ-cells. When, however, we remember that protoplasmic continuity is probably maintained through all nucleated cells in conjunction, that the threads unite the nuclei into a community, and that the cell-walls themselves are, so to say, saturated with living protoplasm, too; and if germ-plasm accompany this universally distributed protoplasm, we seem to have the very machinery required for the purpose. Thus can we hypothetically account for the fact that flowers with germ-cells can be produced from any part of the vegetative system, and that any specific characters, such as roots, bulbs, and tubers, may be produced abnormally on the aërial parts of plants; and, lastly, that the power of reproducing any acquired characters may be transmitted to the germ-cells, and consequently such may be then hereditary.

If the theory, as represented in Dr. Weismann's work, is thus shown to be insufficient to account for hereditary traits of plants, as seen in the vegetative system, there remain the reproductive organs which constitute the flower.

As these, and these alone, contain germ-cells, it would seem that they are the only organs which one would suppose, *à priori*, capable of being influenced by the direct action of the environment. The application of the theory to *The Origin of Floral Structures through Insect and other Agencies*, is, therefore, apposite.[14]

Dr. Weismann observes with regard to the localisation of germ-cells:—"The whole individual is composed of the millions of somatic cells by which the reproductive cell is surrounded" (p. 75), and again, "the number of somatic cells becomes so large that they surround the reproductive cells with many layers" (p. 77).

This description does not apply to plants. The egg-cell of the embryo-sac, and the sperm-cell of the pollen-grain are close to the surface. Thus light can penetrate to the ovule and turn it green, and one layer of cells only separates the pollen from the atmosphere.

In my work just quoted I endeavoured to show that the infinite variety of adaptations to insects discoverable in flowers may have resulted through the direct action of the insects themselves, coupled with the responsive power of protoplasm. I cannot verify the deduction experimentally, for it is impossible to make a flower,

[14] This is the contention of my work with the above title. *Internat. Sci. Series*, vol. lxiv.

except indirectly;[15] but I "proved" it by appealing to the argument of the accumulation of probabilities, based on innumerable coincidences and correlations.

Now, however, Dr. Weismann has provided me with an unlooked-for support, supposing such to be necessary, because he insists upon the action of the environment being directly on the germ-plasm of the germ-cells. He thus speaks:—"There only remains one principle by which we can explain the transformation of species—the direct alteration of the germ-plasm" (p. 388), or "the direct change in the germ-plasm affected by external influences, using these words in their widest signification" (p. 411), and "we must include everything which is not germ-plasm" (p. 396).

In flowers visited by insects, the ovules and pollen-grains are both prepared and ready at the very time the insect arrives. Its irritating action is directly upon the anthers and stigma, which is in immediate communication with the ovule, embryo-sac, and egg-nucleus; so that it is impossible to conceive of a closer contact than that which thus obtains between the insect and the germ- and sperm-cells.

If, therefore, it be necessary, in order that the peculiarities of flowers should be hereditary, that the influence of the irritations be directly upon the germ-cells, then the conditions for securing such are exactly those which every flower that is in adaptation to insect fertilisation supplies.

With regard to the origin of new specific characters by sexuality, Dr. Weismann observes that "permanent hereditary variability may follow from the varied combinations which are due to the mixture of two individually distinct germ-plasms at each act of fertilisation" (p. 327). And again: "Variability depends upon sexual reproduction" (p. 293).[16]

When, however, we perceive the constancy of specific characters of plants growing wild, Nature does not seem to supply any encouragement to the acceptance of this idea. Not only are the species of plants named by Linnæus just as they were in his day, but Dr. Weismann calls the reader's attention to the fact that species of plants in Egypt have remained unchanged for thousands of years; while he refers also to arctic plants, now widely dispersed, which are probably very much the same as they were at the Glacial Epoch.

On the other hand, had he alluded to varieties and hybrids, there would have been more to be said in his favour, as the successive generations of such, under cultivation at least, vary immensely; but their variations mainly refer to size and colour, and only to a limited degree in form, *e.g.*, "Pelorian" varieties.

[15] See *op. cit.*, chap. xiv., on "Acquired regularity and Peloria," p. 128.

[16] A theoretical description is given on p. 321.

The great variability under cultivation Dr. Weismann refers to "Panmixia," or the cessation of Natural Selection, with the consequence of indiscriminate crossing. He thus describes it:—"As soon as natural selection ceases to operate upon any character, structural or functional, it begins to disappear" (*Note*, p. 140). If so, and if Natural Selection is withdrawn under cultivation, thus ceasing to keep up any (and that is equivalent to saying *all* the) characters of plants to the required standard, then *all* the organs of a plant ought to degenerate and atrophy. But hypertrophy and not atrophy is the most obvious characteristic of all cultivated plants.

Again, he says:—"As soon as an organ becomes useless, the continued selection of individuals in which it is best developed must cease, and *panmixia* takes place" (p. 291).

It is not easy to find cases of degeneracy in cultivated plants, but the two following may suffice. In *Bellevallia comosa* the terminal flowers of the raceme are already abortive in the wild state. Under cultivation they *all* deteriorate and become rudimentary, or vanish altogether, but when this stage is arrived at, the flower stalks, now absolutely useless, become subject to hypertrophy, so that a large feathery process of violet-coloured pedicels is obtained. Similarly in the Guelder-rose, as soon as the central and normally perfect flowers have lost their sexual organs under cultivation, the corollas, now useless to attract insects for fertilising the pistils, become increased in size, until a great globe of barren flowers is the result. According to Dr. Weismann, both the flower-stalks of *Bellevallia* and the corollas of *Viburnum opulus* should have atrophied and disappeared. Similarly we ought not to have any "double" flowers at all, and garden roses should be a thing unknown.[17]

Lastly, by panmixia there "follows a mixture of all possible degrees of perfection, which must in the course of time result in the deterioration of the average development of the organ" (p. 292).

In making this somewhat categorical statement, Dr. Weismann does not appear to have allowed for the prepotency of the healthy parent. Thus, a plant which begins to show deterioration by one or more of the stamens becoming petaloid, if it be crossed by a healthy "single" flower, will produce offspring with single flowers only.

[17] Mr. Romanes, on reading my MS., has appended the following interesting observation:—"I agree that Weismann is wrong in saying that cessation of selection must always, and in the first instance, produce *atrophy*. What it must always produce is *degeneration*. But the degeneration may, first of all, have reference to the breaking down of *organisation* (mutual co-ordination of parts, &c.) which had been previously formed and maintained by selection. Now, the breaking down of organisation need not entail atrophy, and is even compatible with the hypertrophy of some of the previously co-ordinated parts. The whole apparatus being thrown out of gear, nutriment before supplied to the vanishing structure may be supplied in excess to the others, and all kinds of mal-correlations of growth ensue. In a state of nature, however, the *reversal* of selection will prune away all such useless growths."

Similarly a weak or tender constitution may be strengthened by crossing with a more robust one, &c., so that the effect is not to lower the standard of the whole of the offspring (as if the weaker one were prepotent), but the superior strength of the healthy parent raises them up to its own standard, and obliterates the downward tendency.

A few concluding words on the nature of proof. Dr. Weismann says:—"My theory rests, on the one hand, upon certain theoretical considerations. . . . On the other hand, it rests upon the want of any actual proof of the transmission of acquired characters. My theory might be disproved in two ways—either by actually proving that acquired characters are transmitted, or by showing that certain classes of phenomena admit of absolutely no explanation unless such characters can be transmitted" (p. 388).

The reader will judge whether I have not sufficiently fulfilled both these requirements of proof. He speaks of the belief in the transmission of acquired characters being "vaguely felt." It is not so with botanists.

It is based, first, upon a deduction of identically the same kind as of Geology and Astronomy, viz., the vast accumulation of coincidences and correlations, which preclude the converse probability. Indeed, Dr. Weismann recommends the use of this line of argument himself, for he observes that the doctrine of evolution is based upon it, so that "it may be maintained with the same degree of certainty as that with which astronomy asserts that the earth moves round the sun; for a conclusion may be arrived at as safely by other methods as by mathematical calculation" (p. 255).

The deductions of geologists cannot be verified by experiment, because we cannot make Nature retrace her steps and show us how the Oolitic rocks were laid down, or how the Devonian fishes came into existence.

Similarly, astronomers cannot secure a portion of the sun for chemical analysis, but we believe we know something of its composition by spectrum analysis, which furnishes a great number of coincidences, but nothing more.

With plants, however, not only is the belief based on innumerable coincidences and correlations, infinitely more numerous than the spectrum can furnish in the case of the sun, but experimental evidence, whenever it has been tried, as with aquatic, subterranean, maritime, spinescent, and hypertrophic structures, &c., as I have shown, invariably corroborates, and thus verifies the deduction.

Lastly, we must never lose sight of the fact that Dr. Weismann does not pretend to offer anything more for our acceptance than an "assumption" (p. 197), or a "theoretical consideration" (p. 388), or an "imagination" (p. 174). He admits that no microscope has ever revealed the nature, or we may add, the existence of germ-plasm, or proved it to be as complicated as he assumes (pp. 168, 191, 271).

Nor can it be distinguished from idioplasm (pp. 180, 200). Nor, we may add, can either one or the other be practically differentiated, but only by the imagination, from what is known as protoplasm, which, however complicated its structure may prove to be, appears to be perfectly capable of acquiring new characters and fixing them by heredity.

GEORGE HENSLOW.

II.

The Poisonous Secretion of Batrachians.

THE author of this review would regret if, by drawing attention to the toxic properties of the Batrachian integument, he were to revive the former dread of a most interesting and useful group of animals—a feeling which, although still so prevalent, is gradually abating among the educated class. For it must be borne in mind that, although every species, even including the frogs, possesses a greater or lesser amount of poisonous properties for mere protective purposes, no Batrachian has the gift of spontaneously squirting out its poison. No more serious accident than a slight irritation of the mucous membranes can be caused by gently handling even the most formidably endowed members of that group of animals.

As remarked by Brunton (4), however, animal poisons are at present acquiring a new interest, from the relationship now found to exist between them and the poisons formed by disease germs, and we therefore think some of the readers of this journal will be pleased to find a *résumé* of the present state of our knowledge of Batrachian poisons as a sequel to the short communications which have recently appeared in the correspondence columns of the *Lancet* (4, 15). The tolerably complete bibliographical index appended to this article will enable those specially interested in the matter to refer to the sources from which it has been compiled.

The skin of the dorsal surface of all Batrachians is studded with glands and follicles. In various species, the Toads and Land Salamanders for instance, some of the larger glands appear as very prominent warts pierced with wide pores, especially a large mass situated on each side of the back of the head behind the eyes (see Fig. 1, next page), which has received the misnomer of parotid, a name now generally changed to parotoid gland. These glands secrete, more or less abundantly according to the species, a viscid or milky, more or less and variously odorous fluid, which is exuded when the animal is disturbed, by means of the contraction of small muscles disposed in the skin around the glands (Leydig, 18). If the larger glands be pressed or subjected to an electric current, the secretion is squirted out with great strength, and sometimes to a considerable distance. Kobert (16) recommends, as the best means of obtaining an abundant supply of toad-poison, the subcutaneous injection of a small dose of chloride

of barium; after a few minutes the animal is so covered with its secretion as to appear as if coated with whitewash. The object of the secretion is to guard Batrachians from the attacks of various enemies. A dog will not often repeat the experiment of seizing a toad; the signs of pain which it evinces after the poisonous fluid has come into contact with its mouth is familiar to most of those who have kept young dogs in the country. That the toad's poison does not protect it against snakes is well known, our common snake, for instance, not feeling any more repulsion for it than for a frog, although it will not eat such Batrachians as *Bombinator* and *Salamandra*. I myself have witnessed the discomfort of a hungry specimen after having seized a Land Salamander, which it very soon let go. But even the Salamander is not proof against one kind of snake at any rate, the common viper. It is also well known to all who have handled

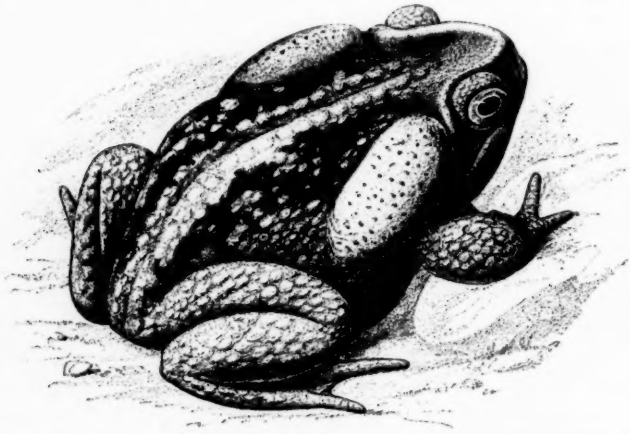

FIG. 1.—*Bufo marinus*, a South American Toad with enormously developed parotoid glands.

freshly-caught Newts, and certain toads, especially *Bombinator*, that the secretion of these Batrachians acts as a sternutatory, and causes irritation of the mucous membrane of the nose and conjunctiva; the effects produced on us by *Bombinator* being comparable to the early stages of a cold in the head. German violinists, it is said, when suffering from moist hands, are accustomed to check the perspiration by handling live toads. Many collectors of Batrachians have learned, to their discomfiture, how the introduction of examples of certain species in the bag containing the spoil of their excursion may cause the death of the other prisoners; for although the poison has no effect on the skin of individuals of the species which produces it, different species, however closely allied, may poison each other by mere contact. We shall see further on that, when ingurgitated or inoculated, the poison acts even on the individual by which it has been produced.

Miss Ormerod (19) thus relates a personal experiment with the exudation of the Crested Newt:—"For the sake of exactly ascertaining the sensations (which in the lower animals could only be judged of by their apparent effects), a part of the back and tail of a live *Triton* were gently pressed between the teeth sufficiently to alarm the animal and cause it to give out its acrid cutaneous exudation. The first effect was a bitter astringent feel in the mouth, with irritation of the upper part of the throat, numbing of the teeth more immediately holding the reptile, and in about a minute from the first touch of the newt a strong flow of clear saliva. This was accompanied by much foam and violent spasmodic action, approaching convulsions, but entirely confined to the mouth itself. The experiment was immediately followed by headache lasting for some hours, general discomfort of the system, and half an hour after by slight shivering fits."

Contrary to what Rainey (24) and Davy (7) were still inclined to believe as late as 1863, the secretion of Batrachians possesses more than an irritating quality; it is a very active poison when introduced into the circulation, as has been proved by repeated experiments from the time of Laurenti (17) down to the present day. It is principally with the researches of Gratiolet and Cloez (13, 14), Albini (1), and Vulpian (26, 27) that the study of the subject entered its truly scientific phase; and experiments have since been pursued by Zalesky (28), Fatio (9), Fornara (11, 12), Calmels (5), Dutartre (8), Phisalix (20, 21, 22), and others. All these experiments have shown the poison of toads, salamanders, and newts to be capable of killing mammals, birds, reptiles, and even fishes, provided, of course, the dose be proportionate to the size of the animal inoculated. It is unnecessary to give here particulars of the experiments, for which the reader is referred to the publications of the authors quoted above; it will be sufficient to say that small birds and lizards succumb, as a rule, in a few minutes, and guinea pigs, rabbits, and dogs in less than an hour.

The notion first entertained by Vulpian (26) that Batrachians are proof against their own poison, has been disproved by Claude Bernard, and subsequently by Vulpian himself (27), Capparelli (6), and Phisalix (20). Toads and salamanders inoculated with their own secretion perish; but this effect is only produced by using a stronger dose than is required for killing individuals of another species. In his elaborate contribution on the poison of the Crested Newt, Capparelli (6) states that he succeeded in killing specimens of that Newt by introducing the secretion in the buccal or abdominal cavities; he also shows that the poison produces very different effects, according to its being introduced into different parts of the organism.

The poison of Batrachians, unlike that of Snakes, is not a septic, but acts upon the heart and the central nervous system. That of the common toad has been compared, as regards its effects, to *Digitalis* and *Erythrophlæum*. For a long time authorities disagreed as

to the nature of the secretion of toads and salamanders. Gratiolet and Cloez at first (13) regarded it as an acid, but in a later paper (14) pronounced it to be an alkaloid, which view was also held by Zalesky (28). Quite recently, Phisalix (21) claims to have solved the question by reconciling both views: toads and salamanders are simultaneously possessed of two different kinds of glands, different both anatomically and physiologically. These are mucous or slime glands, spread over the greater part of the body, the exudation of which is controlled by the animal; and specific glands (the parotoids and the larger dorsal glands), the product of which can only be squirted out by foreign agency. The secretion of the former glands is an alkaloid, and acts as a narcotic; that of the latter is an acid, and acts as a convulsive. The anatomical differences of the two kinds of glands have been worked out by Paul Schultz (25), who, however, regards the product of the slime-glands as innocuous.

The frogs, so sensitive to the poison of other Batrachians, are, nevertheless, not free from toxic properties. This fact, first noticed

FIG. 2.—*Dendrobates tinctorius* (*Phyllobates chocoensis*, Posada-Arango).

by Vulpian (27), has been placed beyond doubt by the experiments of Paul Bert (3). A goldfinch inoculated with the secretion of the edible frog died in one minute, a frog of the same species in one hour and a quarter.

We still know very little with regard to the poisonous secretions of exotic Batrachians. Lacerda Filho (10) has experimented upon the common toad of Brazil, and Posada-Arango (23) informs us that the Indians of Colombia employ the secretion of a small frog, *Dendrobates tinctorius* (renamed *Phyllobates chocoensis* by that author), for poisoning their arrows. The exudation is obtained by exposing the frog to a fire, and after being scraped off the back is sufficient for poisoning fifty arrows. Posada-Arango's experiments on small mammals and birds have shown the poison, which he calls *Batracine*, to act on the central nervous system, like that of the Salamander, the *Samandarin* of Zalesky, or *Salamandrine* of French authors, not on the heart like toad-poison, for which he uses the name of *Bufine*. The name

Phrynine has since been proposed for the latter (Fornara, 12). Claude Bernard (2) had previously alluded to the fact that the Indians of South America use Batrachian poison, with the same effect as curare, for poisoning spears and arrows which are specially used against monkeys. This *Dendrobates tinctorius* (Fig. 2) derives its name from the property it is said to possess of "dyeing" parrots. The method employed for that purpose consists in removing part of the feathers of green parrots, and injecting the bare skin with the blood of the Batrachian; the feathers are regenerated green or yellow on these spots, and artificial varieties are said to be thus obtained.

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III.

The Evolution of Flat-Fishes.

IN starting to deal with any biological question it is well to plant our feet firmly on the solid ground of fact. In the present instance we can ascertain, without much risk of ambiguity, what are the structural peculiarities of the Flat-fishes. All we have to do is to compare them carefully with various symmetrical fishes. The most important and most obvious peculiarities are (1) the position of the eyes, both on the upper side of the body; (2) the absence of pigment from the lower side; (3) the anterior extension of the dorsal fin separating the blind side of the head from the eyed side. To these may be added a fourth, which is less important, being more marked in some genera or species than in others, and in some apparently absent, namely, the greater development of the jaws and teeth on the lower side. Mere dissection of adult specimens shows that the anomalous position of the eyes is due to a distortion of the facial region of the skull. The cranial region of the skull is but slightly altered, but the interorbital parts of the two frontal bones are bent away from their original position in the dorsal median line down to the side of the head, and they are also compressed into a thin plate. But the eyes have pretty nearly the same relations to the interorbital septum as in an ordinary fish. There is an eye on each side of the interorbital septum, as usual.

It is, in fact, the curious condition of the dorsal fin in the flat-fish, even more than the mere distortion of the eyes, which makes it so different from an ordinary fish. If the fin terminated some distance behind the eyes, or if it was prolonged in the direction which it ought to follow, that is along the line which divides the two frontal bones from one another, it would be plain at a glance which was the left side of the head and which the right. It would then be obvious that the left eye was still on the left side of the head, and the right eye on the right. But the dorsal fin does neither of these things. The ectethmoid bone belonging to the blind side is much enlarged, and sends back a process outside the eye belonging to that side to meet another process from the cranial region of the skull. Thus the eye which has migrated, the upper eye when the fish is held in a vertical plane, is enclosed in a complete bony orbit, while the lower eye is merely bounded on its outer side by the jaw-muscles.

It is on this bony bridge, entirely foreign to the anatomy of an ordinary fish, that the dorsal fin supports itself in its advance towards the snout. Properly speaking, the left side of the face in a plaice, for instance, extends from the ventral edge, or chin, to the line between the eyes; but the dorsal fin in its anterior extension divides this side of the face into two parts.

The colour of the skin in a flat-fish is the combined effect of three distinct kinds of tissue. On the inner side of the skin is a dense continuous opaque layer, with a white reflecting surface; this may be called the *argenteum*. Near the outer surface of the skin are two sets of microscopic elements—the *iridocytes*, which are fixed in form, and, like the argenteum, opaque and reflecting, and the *chromatophores*, which contain pigment, and are able to expand and contract their radiating processes. Both argenteum and iridocytes are present on the lower side as on the upper; it is only the chromatophores which are absent on the lower side.

The origin of the structural peculiarities in each individual Pleuronectid by a process of metamorphosis is also within the region of verifiable fact. In the majority of genera the lower eye moves apparently round the edge of the head, remaining always at the surface, and always functional. When the transformation takes place in this way the dorsal fin terminates behind the region of the eyes up to the moment when the lower eye has reached the upper side. Then it advances and, as it were, cuts off the retreat of the eye which has (apparently) migrated. In certain cases observed by Steenstrup and Alexander Agassiz the dorsal fin extends between the eyes, while there is still one on each side of the head.

The lower eye then seems to pass through the head to reach the upper side. Steenstrup, through insufficient knowledge of the anatomy of the adult flat-fish, concluded from this observed passage of an eye through the head that the lower eye passed under the frontal bone of its own side to reach its final position. But Agassiz, with truer insight, explained that the eye merely passed through the base of the dorsal fin, which in this particular instance (the genus *Plagusia*) extended forwards before the change in the position of the eyes, instead of after that change.

The disappearance of the chromatophores from the lower side of the flat-fish also takes place during the individual metamorphosis. The larva has chromatophores on both sides, as it has an eye on each side; but when the fish has taken to lying on one side on the bottom, the chromatophores on the lower side disappear.

These primary and essential facts of the peculiarities of the adult flat-fish and their development in the individual are at the present day well-known, and generally accepted. When we pass on to the deeper question of the explanation of these facts, we can likewise formulate certain broad propositions which no modern zoologist will dispute. But the statement that flat-fishes were evolved from

symmetrical ancestors, and that their individual metamorphosis is a recapitulation of their secular evolution, is now a mere truism. We have to consider the causes of the evolution and the causes of the metamorphosis.

There are doubtless some whose minds are satisfied with the purely orthodox Darwinian explanation. The theory is so simple and so universal, and it is not only so much easier to believe than to doubt, but it saves so much trouble to have one key to all cases of evolution, instead of having to find a special key for each by laborious research and experiment. What can be more obvious than that the peculiarities of the flat-fish are beautiful adaptations to its peculiar mode of life? It is certain that variations of the most diverse kinds occur; there are the variations of these very peculiarities in the flat-fishes to prove it. What other explanation is required than the survival of the most adapted individuals in each succeeding generation while the less adapted were continually slain in the never-ending battle of life?

Well, perhaps the weakest point in this argument lies in the fundamental assumption that all the peculiarities of the flat-fish are adaptations. Weismann says "alles ist angepasst." There must be no quibbling here as to the meaning of adaptation or *Anpassung*. The Darwinian theory depends for its force on the assumption that every character of an organism is or has been of some definite advantage to it in its endeavours to secure its own existence or to propagate its species. What then is the advantage to the flat-fish of the absence of chromatophores from the side on which it rests? No naturalist has ever ventured to suggest that it uses the conspicuous whiteness of its lower side in courtship or in obtaining food, and it is certain that when alarmed every flat-fish hides this lower surface completely, burying itself in the soft ground on which it lives. The Darwinian may reply that no positive advantage need in this case be assumed, that the chromatophores have a special function in concealing the fish by changing its tint, and that being useless on the lower side they have disappeared from absence of selection and economy of organisation. Why then have not the iridocytes and the argenteum also disappeared, since these have much to do with the coloration of the upper side?

This question of the absence of colour, that is, of chromatophores, from the lower side of flat-fishes has given rise to the most curious misconceptions. The observations of Alexander Agassiz, made only fourteen or fifteen years ago, on the metamorphosis of these fishes, constituted an epoch in the history of our knowledge of the subject, for they reconciled the apparently contradictory conclusions of Steenstrup and Malin concerning the migration of the lower eye. But in the same paper in which Agassiz described the transformation followed by him for the first time in living specimens, he has hopelessly confused the two entirely distinct questions of the changes

of colour in the upper side of flat-fishes and the absence of chromatophores from the lower side. He might conceivably have been able to prove that there was a connection between these two phenomena, but he has not attempted to do this. He refers to Pouchet's paper on the *Changements de Coloration sous l'Influence des Nerfs*[1] as indicating such a connection. He states that Pouchet studied chromatophores especially in relation with the "atrophism of the colour on the blind sides of Flounders, pointing most plainly to the partial atrophy of the great sympathetic nerve, effected during the passage of the eye from the right to the left, or *vice versâ*, as the cause."

It will, perhaps, seem incredible, though the truth of the statement can easily be verified, that Pouchet, in the paper to which I have referred, says not a word concerning the cause of the absence of colour from the lower side of the Pleuronectid. It is strange, indeed, that the distinguished son of the great author of the *Poissons fossiles*, whose native language was French, should have fallen into such an error in quoting a French memoir. Agassiz appears to have forgotten for the moment that the optic nerves in a fish cross one another at their origins from the brain, so that the eye of the right side is connected with the left half of the nervous system and *vice versâ*. Pouchet particularly describes two experiments upon turbots made expressly to ascertain whether one eye had more influence than the other on the chromatophores of the upper or left side. It is well known that Pouchet found the power in a fish of accommodating the general tint, *i.e.*, the lightness or darkness of its skin, to that of the ground on which it was placed, to depend upon the eyes. When both eyes were removed this power of accommodation was permanently destroyed; the chromatophores were unable afterwards to contract or expand. He found that the removal of one eye in *Gobius niger* did not affect this power of accommodation—the chromatic function—in the least. He removed the right or upper eye of one turbot, the left or lower eye of another, and found that in both cases the chromatic function was uninjured; the upper side continued to grow light or dark according as it was placed on light sand or dark mud.

Pouchet, however, further demonstrated by experiment that the connection between the eyes and the dermal chromatophores was effected exclusively by the great sympathetic nerve cord or chain of ganglia. If the sympathetic plexus in the inferior vertebral canal of a turbot was divided by cautery, the chromatophores of the skin behind the section were paralysed. Agassiz may have supposed that although both eyes were connected with each sympathetic, yet the migration of the *lower* eye in some way affected the sympathetic of the lower side, and that each sympathetic was connected with the chromatophores only of its own side. But Pouchet says nothing about all this, and Agassiz makes no mention of any evidence that the alteration

[1] *Archives de Physiologie et d'Anatomie*, 1876.

in the position of the eyes in the flat-fishes affects in any way the sympathetic nerve of the lower side.

Agassiz remarks in a foot-note that Pouchet has succeeded in producing a white side in trouts by destroying the eye *of that side*. As authority for this, he refers to an article in the *Revue Scientifique* for 1877. I have not seen this particular article of Pouchet's, but I have before me the latter's own abstract of his experiments on the trout as described in two papers published by the *Société de Biologie* in 1876 and 1878. He states in this abstract that by taking a trout from a river where the bottom was in some places light sand and in others dark weed, and destroying one eye, he was able to provoke a unilateral change of colour. The side *opposite to that from which the eye was removed* became permanently dark, the chromatophores being paralysed and expanded. He says that the crossing of the optic nerves being complete, one can conclude from this experiment that the great sympathetic of each side depends directly on the corresponding half of the brain. But this result was never obtained in flat-fishes. The difference between Pouchet's investigations and Agassiz's description of them could not possibly be greater. Pouchet does not even refer to the question of the origin or the atrophy of the chromatophores; Agassiz supports, as though it were Pouchet's, a theory that the stimulation of the chromatophores by reflex action through the eyes might not merely produce pigment, but pigment of colours similar to the colours of the light acting upon the eyes—that is, to the colours of the surroundings of the fish—and thus explain the origin of what he calls mimicry—in more modern language, protective resemblance. He puts aside the idea that the absence of chromatophores or of pigment from the lower side of flat-fishes is in any degree due to their withdrawal from the direct action of light. He states that he placed young flounders in a glass dish at a height over a table so that light could reach them from below, and that this arrangement neither prevented the transfer of the eye nor produced any effect in retaining the pigment spots of the blind side longer than in specimens struck by the light only normally from above.

I have made a series of careful experiments of this kind with young flounders taken at the beginning or in the middle of their metamorphosis, improving on the method followed by Agassiz, by placing a mirror below the aquarium at an angle of 45°, and cutting off the light from above by means of an opaque cover. In the *Zoologischer Anzeiger* of 1891 I published the result of the first of these experiments, namely, that in the great majority of the specimens treated in this way, after several months, although no effect was produced upon the eyes, more or less of the skin of the lower side was pigmented.

This experiment of mine has attracted the attention of Professor Alfred Giard, who criticises it in a recent note published in the

Proceedings of the *Société de Biologie*. Giard has considered the evolution of the Pleuronectidæ before, although he has not made a special study of their anatomy or development. Years ago he suggested a new theory in explanation of their origin, to which he refers in this note. His familiarity with the problem, as well as his studies in the doctrine of evolution generally, caused me to expect to find his remarks both sound and suggestive; but I must confess to disappointment. The immediate occasion of this note was the capture of what is commonly known as a double turbot. The right eye in this specimen was on the edge of the head, so that the dorsal fin, instead of extending as it usually does to the anterior part of the head, was separated therefrom by a concavity. The right side was pigmented all over like the left, but the colour was a little less intense. The specimen was 19 cm. long.

In almost every ichthyological work of any importance the occurrence of double flat-fishes having the characters just mentioned is described. Yet Giard proceeds to remark that the relative frequency in *Rhombus maximus* of this monstrosity, which has not been stated to occur in other Pleuronectids, is due without doubt to the fact that, as Day and McIntosh have pointed out, the young turbot swims in a vertical position for a longer time than the other flat-fishes. It is perfectly well known that double specimens occur in many species of flat-fish. They are as common in the flounder as in any species, and a specimen of this species is described and figured by Traquair in his memoir published by the Linnean Society in 1865. I have in my possession adult specimens both of the flounder and the plaice which exhibit this monstrosity. Day himself, to whom Giard refers, states in his "British Fishes" that double specimens have been observed in the flounder, turbot, plaice, soles, &c. Ritzema Bos records a case in the flounder in the *Biologisches Centralblatt* of July, 1886. There is, therefore, no evidence whatever that the monstrosity in question is more frequent in the turbot than in other species.

After describing his abnormal specimen, Giard remarks: "L'animal *devait* nager en se tenant verticalement et se poser rarement sur le côte droit." *Il devait nager*, the fish must have swum in a vertical position, and must have rested rarely on its right side. The question is, did it swim vertically and lie on its side rarely? Day states that these abnormal double specimens are seen to swim vertically, and to be more frequently found near the surface of the water than those which progress in the usual manner. He gives no actual observations in support of this statement. I have for some time past been endeavouring to obtain evidence on this particular point. Apart from observations on living adult double specimens, I have good ground for believing that they do not swim vertically, for I have observed living young flat-fishes many times during the period of their metamorphosis, and I know that they never swim vertically after the moment when the eyes have ceased to be perfectly

symmetrical. When the lower eye has begun to move towards the edge of the head, the body is held in a slanting position, and throughout the metamorphosis the line joining the two eyes is always kept horizontal, so that when the lower eye is on the edge of the head the body is at an angle of about 45° or more with the vertical. This is particularly well seen in the young turbot, which possesses a swimming bladder, and never seeks the bottom at all until the metamorphosis is almost complete. Giard quotes McIntosh as witness that a young turbot, 9 millimetres long, with its eye on the edge of the head, swam in the vertical position. This is simply an error of observation.

We have, however, no satisfactory evidence at present that the monstrous specimens, whose metamorphosis is permanently arrested, swim about in whatever position any more frequently or rest on the bottom less constantly than their normal brethren. I have now under observation a living double specimen of the plaice; its eye is on the edge of the head, the dorsal fin terminates behind the eye, and the posterior three-fourths of the lower side are coloured like the upper, the anterior fourth being white. This specimen, instead of showing a tendency to continue swimming in the water, cannot even be induced to leave the bottom long enough to enable me to see whether it holds itself perfectly horizontal or not. I have never seen it leave the bottom of its own accord; it lies always buried in the sand up to its eyes, and, when disturbed, makes violent struggles to bury itself again. I have had this specimen for more than six months.

In spite of all this, Giard not only assumes that double specimens swim vertically, but actually maintains that the existence and assumed habits of these double monstrosities is better evidence of the dependence of pigmentation upon light than the result of my experiment. "Dans ce cas comme dans beaucoup d'autres, la nature réalise pour celui qui sait la comprendre, une expérience bien plus parfaite que celles que nous pouvons imaginer dans nos laboratoires." For Giard goes on to remark that it was only at the price of a thousand difficulties that I succeeded in showing, in an incomplete fashion, that pigment appeared on the lower side of the flounder when light was directed upon it, and that the experiment is not absolutely conclusive. He detects a fallacy in my evidence, and this part of his short paper is still more remarkable than the rest. He points out that, of all the flat-fishes, it is in the flounder that reversed specimens are most common, that is, specimens in which the white side is the right, and the eyes are on the left, instead of *vice versâ*, as in normal specimens. He argues that the specimens upon which I experimented may have numbered reversed individuals among their ancestors, and the pigmentation adduced in evidence by me may have been due to inheritance from one of these ancestors which was pigmented on the left side. The probability is the greater in that double individuals are not rare

among flounders taken from the sea.[2] The pigmentation in my experiment may even, he suggests, be sufficiently explained as a souvenir of the original bilaterally symmetrical condition. He magnanimously mentions, however, that the individuals kept by me under the ordinary conditions remained white on the blind side, which is a fact that I stated, and he also adds that reversed specimens must be rare at Plymouth, since I have not even anticipated the objection, which is an erroneous inference. At Plymouth reversed flounders are exceedingly common, almost as abundant as normal specimens. It is true the objection did not occur to me, for it is obvious that if inheritance in the flounder acted as Giard supposes, there would be none but double flounders, since right-sided and left-sided individuals are always breeding with one another. It is remarkable that Giard does not perceive that his objection is more applicable to his own explanation of the ambicoloured turbot.

In conclusion, Giard reiterates his belief in his own theory, that the asymmetry of the Pleuronectidæ is due to the inequality of the eyes in their pelagic larvæ. This theory was announced in 1877, *à propos* of Agassiz's researches. The kernel of the theory appears to be contained in this sentence:—"From the moment when one eye is stronger than the other there must be a rotation towards the more feeble side, and a displacement of the (stronger) eye which can see through the body on account of its transparency and slight thickness: little by little selection reduces the amount of material between the eye of the blind side and exterior objects," and so on. I confess I do not understand Nature sufficiently to be able to appreciate this theory. Transparent pelagic animals have sense organs, especially eyes, according to Giard, of unequal size, but he does not say why. As a matter of fact, the eyes in young flat-fishes are not unequal in size—at least I have looked for any such inequality in vain. It is true that Owen states that the optic nerve of one side is thicker than the other in the halibut; but I have recently dissected the head of a halibut, and found no difference in thickness between the two optic nerves. It seems to me that even if one eye were larger than the other, this is no reason why it should move out of its place; and I imagine the mere transparency of the body could not produce inequality in the size of the eyes. If I understand Giard aright, the flat-fish lies on its side because its eyes are asymmetrical, whereas it is obvious to other naturalists that the eyes are asymmetrical because it lies on its side.

It is not my purpose on the present occasion to set forth at length the views I agree with or hold on the subject I have dealt with. I have intentionally devoted this opportunity to destructive criticism. I have considered some examples of the loose and

[2] Giard evidently supposes that the abnormality of the eyes does not occur in double flounders, since on the preceding page he states that it is confined to the turbot.

irresponsible fashion in which the deeper problems of Biology are too often discussed. There is too much striving after brilliancy and originality, and far too much importance attached to these qualities. In Biology, at present, it would sometimes seem that the Italian epigram, *se non è vero, è ben trovato*, is the verdict which authors desire for their papers. When the arguments of eminent biologists exhibit such an absence of logic and accuracy, such an abundance of self-contradictory propositions and baseless assumptions as some that have been published concerning the evolution of flat-fishes, it is small wonder that the claim of Biology to rank as an exact science should be questioned.

The physiology of the organism, like the physiology of organs, must be investigated by experiment. The final object of the exact sciences is to trace the connection between cause and effect, to ascertain the antecedent conditions which are not only necessary but sufficient to produce observed phenomena. The rigid and exact methods of experimental science have been learned and applied by the physiologist who studies the functions of organs in the vertebrata. But the evolutionist has still to learn the logical meaning of the word "cause." He seems to think that accuracy and caution are only necessary in observation and description, and that when he leaves these to consider the causes of things observed, he enters a region of romance, where he may soar on the wings of fancy, and where discoveries can be made by a lively imagination without the labour of research.

J. T. CUNNINGHAM.

IV.

Ticks.

IN many parts of the world people are becoming considerably excited just now on the subject of Ticks. It appears to be specially the case in some of the West Indian Islands, but is not by any means confined to that locality. This is not altogether an unusual occurrence, for the little animals are apt to increase in numbers and to bring themselves vigorously into public notice, and they are always more free than welcome. When I say "Ticks," I am using the word in its more scientific sense, as signifying the Ixodidæ, a family of Acarina; not in the popular sense of almost any objectionable small creature which bites and sucks. Thus, for instance, the "Sheep-tick," which is the dread of our farmers, is not only not an Acarid, but it is not even an Arachnid; it is, if one may be excused for using an apparent contradiction, a wingless dipterous insect, somewhat closely related to the curious *Nycteribia* of the bat, and more closely still to the winged *Ornithomyia* of birds.

The true *Ixodes*, however, can make himself quite sufficiently unpleasant without having also to bear the evil reputation of others. No one whose experience is confined to England, and who has only on rare occasions found a minute black speck firmly fixed in his arm when he returns from beating ferns for insects, or has found that his dog has come out of bushes and long grass possessed of somewhat more life than he carried in, has any idea of what a nuisance these creatures can be in tropical or semi-tropical countries. In the West Indies, parts of South America, the southern part of North America, parts of India, &c., they breed in the long grass and bush in countless numbers and of numerous species. Any large vertebrate visiting the infested locality in the Tick season without being most carefully protected, is apt to emerge covered with a quantity of Ixodidæ, which in the case of man is very unpleasant, and in that of animals, which cannot so readily rid themselves of the pest, is even dangerous, both from irritation and, in the case of cattle continually visiting the infested districts, from loss of blood.

The bite of an *Ixodes* in England is practically painless to most people, but it must be remembered that a small wound which would not inflame here may do so in a hotter climate, and the matter is generally greatly aggravated by the means taken to get rid of it. If the Acarid were allowed to suck its fill, it would then withdraw the

rostrum with the terrible-looking armature of recurved hooks by which it is fixed to the skin of its victim, and drop off; but when irritation is felt scratching or rubbing naturally begins, and thus the *Ixodes* is torn away by the simple process of breaking the horny rostrum short off, and leaving the barbed portion in the wound for nature to remove as best she may. Thus, a short time since I received two lots of Ixodidæ almost on the same day. One series was obtained from North Africa, and had been gathered from bats; these had sucked but little, and there was not a single whole rostrum among them. The others, from the Leeward Islands, were fully fed, and had been carefully gathered from cattle, the rostra being unbroken. People, however, naturally do not desire that the Acarid should be allowed to finish its repast in peace, and the question arises, cannot it be induced to withdraw its rostrum before sucking to repletion? M. Mégnin, of Vincennes, who has studied the question, says that it can, and that a drop of turpentine or benzole will cause it to

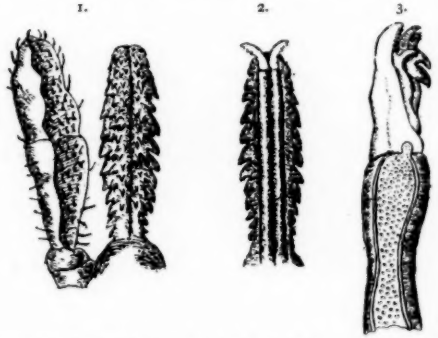

FIG. 1.—Rostrum (maxillary lip) of *Ixodes reduvius*, female (= *I. ricinus*) seen from below mandibles retracted. After Canestrini. One only of the cultriform palpi is drawn.

FIG. 2.—The same part (without the palpus) of *Ixodes lacertæ* seen from above; mandibles protruded. After Pagenstecher.

FIG. 3.—One mandible of *Ixodes reduvius*, female seen from the side. After Pagenstecher.

be so uncomfortable that it promptly removes itself to less unpleasant quarters. I am told that even a drop of oil will have the desired effect in most cases. If there be many Ticks, the question of whether they are allowed to proceed with their meal, or are cruelly removed while still hungry, must be an interesting one to the host, for their capability for sucking is certainly remarkable. When fasting, the male and female are about the same size. The male, however, although not incapable of feeding, like the male gnat, is satisfied with a moderate amount; not so the female, which continues to suck until, in some species, from a size which may almost be described as microscopic, she becomes the greater part of an inch long. Professor Leidy lately had the idea of weighing a considerable number of adult female North American Ticks of one species fasting, and then

weighing the same number of the same species and sex fully fed; and it is startling to learn that the fed specimens weighed more than a hundred times as much as the unfed.

The power of enormous feeding at one time is accompanied, as is often the case, by extraordinary powers of fasting at other times. It has been lately observed by Dr. Strachan, in Jamaica, that young *Ixodes* bred in confinement can live for months without food, and he is inclined to attribute it to their being born with a food-supply; but the knowledge of their ability to live for long periods without food is not by any means new, nor is this capacity confined to the young. Probably it is carried to the greatest extent in the Argasinæ, a sub-family of the Ixodidæ and clearly belonging to them, although formerly, in an unaccountable manner, included in the Gamasidæ by some authors. Some years ago, some dead Acarids were sent to me from Adelaide, Australia, and these proved to be not distinguishable from the well-known *Argas persicus*. As this was an interesting fact, I wrote to enquire if my correspondent could forward some alive, and two or three were accordingly sent; but they were put loosely in a tin box, without food or packing of any kind, nothing in the box except the mites, and in this state they came by post. I was away when they arrived, but on my return, some two months afterwards, I found them all alive. Moreover, numerous young ones had been born, either during the journey or after it, and these also were living. The best instance, however, is that related by MM. Laboulbène and Mégnin. Some time between 1860 and 1870, the Shah of Persia engaged the services of a French physician, Dr. Tholozan, who was a friend of Laboulbène, and was asked by him to examine into the stories concerning the Persian *Argas*, and to send home specimens. In June, 1877, the creatures were sent alive, but without food, packed in cotton-wool, in a box, and wrapped up in paper. Laboulbène sent the box to Mégnin, who mislaid it; just four years afterwards, the box was found and opened. The males had died; the females had had numerous young, which had died; but the females themselves were living, and ready to feed when they got the chance.

This species affords an example of what I said before, namely, that the bite may possibly produce much worse results in a hot country than in England; it was originally described by Fischer, of Waldheim, in 1809, under the name of *Argas persicus*, and he speaks of it as much feared and very dangerous, even destroying life. About ten years later, Maurice Kotzebue, who uses as a popular name "Punaise de Miana," but identifies it with Fischer's *Argas*, gives a terrible account of the creature, saying that its bite produced high fevers, convulsions, delirium, and finally death in twenty-four hours.

Tholozan was not certain whether there was truth in these stories; he found that at Chahroud-Bastam the creatures had a very

bad reputation, and were known by the name of "Guérib-guez," or "strangers beware"; while at Miana there were great differences of opinion, some people—including most doctors—thinking that they were harmless, and that the fevers came from the climate, while others—including most natives—maintained that the fevers arose from the bites of the Acarids. Tholozan sent home specimens from both places. Mégnin tried them on rabbits, and also tried one on his own arm; this bit freely after its four years' fast, and became deep purple instead of dull yellow. For an hour after the creature had left his arm, Mégnin felt as if it were still sucking; for a fortnight there was considerable itching; and six weeks elapsed before the arm was absolutely well. There was not any fever or general illness, but may it not be possible that in a hot and unhealthy climate the wound would have inflamed more, and possibly rendered the patient more susceptible to the unfavourable local conditions, so that there might be some slight foundation for the doubtless greatly exaggerated stories of the earlier writers?

The power of living without food for a long time doubtless explains such cases as the so-called "Canterbury Tick," really *Argas reflexus*, found in the timbers of the roof of Canterbury Cathedral in a place then not accessible to birds, but where it had probably been left by some pigeon years before.

The food of the Ixodidæ is, however, not supposed to be wholly animal. It has usually been considered that they are primarily vegetable feeders who like to indulge in a more generous diet when they have an opportunity, as is believed to be the case with gnats. Of the millions of mosquitoes which in the short Norwegian summer often thicken the air of the Tromsdal—a wild valley within the Arctic Circle, practically uninhabited either by man or probably by beast—how few are likely ever to taste blood? Similarly, it is believed, so far as our present knowledge goes, that the young *Ixodes*, and probably the old ones also in most instances, feed primarily on vegetable juices, only attaching themselves to animals as occasion offers. It seems certain that the eggs are not laid on the animal; the female when her voracious appetite is at last satisfied, *i.e.*, when she cannot contain any more, drops off, and the nutriment serves to supply the store of nourishment to a great mass of eggs which she produces, and deposits on the grass and herbage. The place and mode of deposition is somewhat curious, inasmuch as she places the eggs one by one in front of her in a pile, or round ball, which gradually becomes almost as large as the mother-*Ixodes*. It is probably this position of the eggs which gave rise to the idea held by some of the earlier writers, that the *Ixodes* laid their eggs through the mouth-opening; the fact being that the genital opening is very near the mouth, and the position during oviposition such that the mouth is not readily seen.

The structure of the mouth-organs, which enable the possessor

to fix itself so firmly and also to drop off at will, is interesting. The anatomy of *Ixodes* is not by any means a new subject; from Pagenstecher's almost monographic description, which formed vol. 2 of his "Anatomie der Milben" in 1861, to Professor Batelli's "Comunicazione preventiva" in the *Monitore Zoologico Italiano* for 1891, a good deal has been written about it. I have used the word "rostrum" hitherto for the anchoring organ because that is the name usually given to it; but the appellation is scarcely a happy one, because in other mites it is applied to the whole portion of the body in which the mouth-organs are placed, whereas the anchoring apparatus in *Ixodes* is only one of those organs. It is the homologue of what is often called the labium in other Acari, but which is not in any sense the same as the labium of insects; it is a maxillary lip, answering to the maxillæ of insects or the pedipalps of other Arachnida. It is a paired organ, or perhaps I should say paired organs; but the basal parts of the two sides are broad and flattened, and are fused at their inner edges, so as to form a lower lip closing the mouth and bearing the palpi; the distal parts are rod-like though free, flattened on their inner edges and lying close against each other, but capable of some divarication. On the outer and lower edges these rods are armed with a large number of strong recurved hooks or spines. On the upper surface of this maxillary-lip are two longitudinal channels in which the mandibles play backward and forward. The latter are provided at their distal extremities with elaborate cutting organs, and can be protruded a little beyond the lip. They cut the hole into which the rod-like ends of the lip are pushed; the rods being then slightly divaricated, hold with extreme tenacity; but they also enlarge the hole a little, so that when the creature brings them together again they can be withdrawn, and it can return to its grass-lands and lay its eggs in peace. Another point of interest discovered not long since by Winkler is, that *Ixodes* possesses a heart, or dorsal vessel; and as, with the single exception of *Gamasus*, it is the only heart-like organ known in the Acarina, while its one-chambered form is probably about the simplest arrangement known in the Arthropoda, it is worthy of special remark.

Anyone keeping a large *Ixodes* alive will scarcely fail to be struck by its frequent and wide changes of colour; it is the Chameleon among Mites. An instance of this is related in Sir J. Hooker's "Himalayan Journal," which one would almost hesitate to quote from a work of less authority. Ticks are very common on Lizards, and Hooker found a black and yellow Lizard which had three Ticks on it; one was on a black part, and was black; a second was on a yellow part, and was yellow; the third was just on the line dividing the two colours, and one half of its body was black, while the other appeared yellow.

ALBERT D. MICHAEL.

V.

Physical Geology in the Basin of the Colorado.

THE basin of the Colorado River offers a wonderful series of object-lessons to the physical geologist. The two operations of erosion and crust-movement, in particular, are there exhibited on the grandest scale and in the simplest manner. The region to be noticed comprises the southern and eastern portions of Utah, with parts of the neighbouring States and Territories, Wyoming, Colorado, New Mexico, and Arizona. Utah itself is divided by an ill-defined water-shed, the northern and western parts being included in the Great Basin, which drains only into inland salt lakes. The Colorado River is formed by the union of two great branches, the Green River, which rises in Wyoming and runs south, and the Grand River, which heads in the Rocky Mountains of Colorado, and has a general westerly course. The united waters flow through northern Arizona, in the famous Grand Cañon, and finally reach the Gulf of California.

Physically, the region is a remarkable one. It has, indeed, mountain-chains, some like the Wasatch, the westerly front range of the Rocky system, rising 11,000 or 12,000 feet above sea-level; but essentially it is a country of high plateaux having more or less of the character of deserts. The highest plateaux, bordering the Grand Cañon, sometimes reach an elevation of nearly 9,000 feet.

The phenomena of erosion are intimately connected with the climate, which is an extremely dry one, owing to the great range of the Sierra Nevada, cutting off the moisture from the Pacific. Not only is the annual rainfall very small, but it is concentrated in a few violent storms, leaving the country absolutely parched during the greater part of the year. Except on the highest plateaux and mountains, vegetation is generally restricted to a few hardy forms. The only permanent streams are those fed by the snows of the Rocky Mountains. It would be a great mistake, however, to suppose that aridity of climate implies slowness in denudation. There is every evidence that the general surface of the country is undergoing degradation and removal at a rapid rate. Over large tracts the strata from the Permian to the Eocene inclusive, with an average thickness estimated by Captain Dutton at 10,000 feet, have been completely removed, and the beginning of the process cannot date back farther

than middle Tertiary times. A geologist is at first staggered by the proofs of enormous denudation evident everywhere in a country apparently devoid of water; but the mystery is solved when he has the fate to be overtaken by one of the erratic storms which are a characteristic feature of the region. The whole surface of the plateau is covered with running water; the accumulated detritus of the rocks is hurried along by boiling torrents where an hour ago were only dry gullies; and the larger streams abundantly justify the name which several of them bear—Rio Puerco, the dirty river. The great diurnal variations of temperature have already disintegrated the surface of the rocks; there is no vegetation to receive the direct impact of the rain, and no soil to impede the transport of the material; the rain has thus an enormous power for degradation, a power which is, indeed, a direct consequence of the dryness of the climate.

It is when a tract of homogeneous, horizontally-bedded rocks is subjected to action of the kind described that the typical "Bad Lands" are seen. The country is studded with thousands of bare hills, following one another in close succession as far as the eye can reach, and divided only by a network of dry channels. When harder strata alternate with softer, the simplicity of contour of the hills is modified by projecting ledges, and we have what Major Powell has called "Alcove Lands." The alternation of ledge and talus, or, on a larger scale, of cliff and slope, is strongly marked, and the emphasis with which the stratification thus expresses itself in the surface-configuration is again a consequence of the dry climate, which makes the impact of the rain so formidable a factor in erosion. In a country like England the soil holds the water, which is enabled to effect disintegration in the rocks beneath, whether hard or soft, and the differences of existence are minimised. In an arid region the soft rocks are destroyed rapidly, leaving the more resisting beds in strong relief, and the only limit to this is fixed by the undermining of the ledges, which removes their support.

The traveller on the Rio Grande Western Railway over the two hundred miles from Castle Gate to Grand Junction traverses a magnificent panorama of bad lands and alcove lands, the stratification being here, as is often the case, accentuated by brilliant contrasts of colours in the naked rocks. During most of the journey he has in sight on his left a range of steep cliffs 2,000 feet high, known as the Book Cliffs. These are an example of a feature common in the region —the escarpment cliff, or "cliff of erosion," dividing a higher from a lower plateau. Such abrupt escarpments characterise gently inclined formations of alternating hard and soft characters, the cliff being formed by a hard rock resting usually upon a soft one. There may be two or three parallel escarpment cliffs built of successive formations, as in the plateaux traversed by the "Terrace Cañons" of the Green River. The surface of each platform is approximately a bedding plane, inclining gently towards the cliff which overlooks it.

The surface drainage is in the direction of this slope, and then along the foot of the cliff to one of the permanent rivers. The directions of these permanent rivers bear no such relation to the cliffs, and give rise to an important observation.

Powell has pointed out that when the Green River in its southward course encounters the great range of the Uinta Mountains, lying directly across its path, instead of being diverted, it cuts right through the range and follows its own course. Again, in crossing the Yampa Plateau, the same river cuts directly through the highest ground in the district in a deep gorge appropriately named Split Mountain Cañon. This entire independence of the mountain and plateau systems characterises all the main water-courses of the region. Several rivers rising on the eastern slopes of the Wasatch run directly westward through the heart of the range, and fall into Great Salt Lake. Of such phenomena there is only one explanation; the rivers existed before the mountains and plateaux; in other words, we have a striking example of an "antecedent" drainage system. As an elevation was gradually formed in the path of a river, the latter exercised its most active erosive power upon the part of its bed thus raised, and was able to keep its channel cut down to the proper level, and maintain its right of way. In order that the river should not be diverted, erosion must have been able to keep pace with elevation throughout the whole time, and we have here a clear proof of the very gradual operation of the elevatory forces.

Such movements of elevation as those just indicated have affected in varying degree the whole region, though sometimes the amount of the elevation can be shown to have differed by thousands of feet in adjacent tracts. The rivers, meanwhile, have maintained approximately their original position relative to sea-level. Hence arises one of the most remarkable features of this part of America: the rivers flow in deep narrow gorges or cañons instead of meandering in broad valleys like the streams of a country comparatively stationary. The cañon-type of valley, which is by no means confined to arid regions or to rocks of any particular character, seems to belong especially to regions undergoing uplift, where the rivers, entirely occupied in cutting downward to maintain their grade, have had no energy to spare for eroding laterally.

Bearing this principle in mind, the well-preserved cañon-form of the valleys in the Colorado basin shows that the process of elevation initiated in middle Tertiary times is still in progress. A very striking evidence of this is seen on the abrupt western flank of the Wasatch Mountains. The generalised structure of the range is that of a sharp uplift bounded to the west by a fault of at least 6,000 feet throw. The movement along this fault has doubtless been effected gradually, and a portion of it is clearly of recent date, for where the gorges in the mountains debouch on the great plain of the Salt Lake the fresh fault-scarp is often seen truncating the terminal moraines

of the Glacial Epoch and the still later alluvial cones of the torrential streams.

The uplift of the plateau-districts, as already stated, has been differential. The high plateaux bordering the Grand Cañon stand at different levels. They have horizontal or very gently inclined surfaces, and are separated by cliffs often 2,000 feet in height. Examination has shown that one geological horizon, the summit of the Carboniferous, forms the surface of all these plateaux; consequently the cliffs are actual "cliffs of displacement." In most cases they are fault-scarps, only slightly modified. Sometimes a monoclinal fold is found instead of a fault, the two not being essentially different, but passing into one another. The structure of the country is, then, that of a number of blocks uplifted independently and to different elevations, and bounded by faults or monoclinal flexures. The mountain chains of the region have in general the same kind of structure with more complication and more modified by subsequent erosion. This type of mountain-structure is known to American students of orography as that of the Great Basin ranges. It is characterised by broad monoclinal folds, normal faults, and a form due in the first place to the mountain-building forces. It is strongly contrasted with the Appalachian type with its closely compressed acute folds and reversed faults and a contour due entirely to the agents of erosion. The one type speaks of vertical elevation, the other of lateral compression.

The features of interest to the physical geologist exhibited in the region are too many to be noticed in a short article. In some districts igneous rocks have played an important part in the structure of the country. Besides the typical laccolites of the Henry Mountains, so lucidly described by Gilbert, there are great bodies of intrusive rock connected with much more extensive uplifts. Extinct volcanoes and lava-flows of various ages are seen in large numbers on some of the high plateaux. The lavas are harder than the sandstones and other strata of the district, and so the older flows often form the capping of broad, flat-topped "mesas" standing high above the surface of the surrounding country which had no such protection. Later outbursts of lava on the margin of the elevated tract may extend the protected area, and from successive periods of eruption and erosion arise large mountain-masses consisting externally of lava but internally of stratified rocks long ago removed from the surrounding plateau. Such is the character of the Uinkaret Mountains and of Mount San Francisco, the latter rising nearly 6,000 feet above the plateau on which it stands.

Most of the features noticed above, and many others of equal interest, are typically displayed in the Grand Cañon of the Colorado, an unrivalled example of erosion in an arid climate and in a rising region. Near its upper end it has an average width of ten or twelve miles, with a depth of more than 6,000 feet. Between the

river and the Coconini Plateau above, a succession of life-zones is comprised equivalent to the range from the cactus-plains of Mexico to the pine-forests of northern Canada, the effects elsewhere due to difference of latitude being here given by difference of altitude. In the winter, when the snow lies deep on the plateau, humming-birds flit about among the shrubs in the depths below.

The precipitous form generally associated with the idea of a cañon and finely exemplified here in the many tributary gorges, gives place in the Grand Cañon itself to a more complex structure. There is a wide outer cañon floored by a broad platform, and through this is cut the deep inner cañon at the bottom of which the river runs. The walls of the main trunk are broken by many minor chasms, enormous if considered by themselves, giving rise to deep recesses and long projecting ridges. These branch-cañons again have their tributaries, and when two of the latter have cut back so far as to meet, the salient point is left standing as a great castellated "butte."

Of the spectacle as a whole, no description, however vivid, could give any adequate suggestion. A part of the deep impression it produces is due, as Dutton has well remarked, to the startling suddenness with which it bursts upon the traveller. He may have been traversing for days a gently rolling plain, monotonous almost to weariness, and apparently illimitable: he ascends one more gentle undulation, and, as if raised by a magician's wand, a new world is before him. The endless detail of cliff and talus, butte and alcove, is at first bewildering. The imagination is perplexed for comparisons, but perhaps the dominant impression is that of Titanic architecture. Monuments, pinnacles, buttresses, castles, temples, appear in wonderful diversity, and crowd the distant perspective away to its vanishing point. But soon order begins to appear in the confusion. Guided by the characteristic colour of each formation, buff or crimson, lilac or chocolate, the eye can follow the level bedding into deep amphitheatres and round projecting headlands for fifty or sixty miles. In the broad platform which seemed to be the bottom of the cañon is seen the steep inner gorge, only in places disclosing the river in its channel of dark gneiss. For a juster view we must take our stand on one of the abrupt promontories, such as Spanish Point where Coronado, the earliest explorer of the country, obtained his first view. Here we look westward along the cañon, and the true structure of the whole is clearly revealed.

Far below, at the bottom of the dark inner gorge, rushes the Colorado River, which throughout the two hundred miles of the cañon is a close succession of headlong rapids. As seen from the plateau, its waters, brown from their burden of mud, rise in huge standing waves, a fearsome sight to one who thinks of navigating them. In 1869, Major Powell, now Director of the U.S. Geological Survey, started on a voyage of exploration from Green River Station with a small party in life-boats. Passing down the long

chain of cañons of the Green and Colorado Rivers, he emerged, after fourteen weeks of adventurous travel, at the mouth of the Rio Virgen, below the Grand Cañon. When this voyage through an unknown country was projected, the engineers opposed it as an impossibility. On a river falling more than 5,000 feet in a thousand miles it was held that great water-falls must be met with. Powell was convinced to the contrary: there could be no falls on so muddy a river. In other words, he saw that a stream so intensely active in erosion, as this detritus-laden torrent must be, would always keep its channel cut down to a fairly uniform grade. This foresight is worth quoting as an example of the value of geological reasoning in an untravelled region.

Although there are no falls, there are terrible rapids. The most formidable of these occur where the bed of the rapid river is choked with the huge boulders brought down by its far more rapid tributaries.

The Grand Cañon is the last of the long chain of deep gorges through which these waters flow. Below it, the Colorado emerges into an open country at a lower level, and so pursues its way with more of the usual characteristics of a large river.

It is difficult in any description to avoid emphasising whatever is superficially striking, but the impression of strangeness produced by great magnitudes or unusual simplicity of structure is one to be combated by those who cherish the principles on which modern geology is founded. If the science is built on the laws of Nature, these laws must be the same in the Old World as in the New. Deserts, high plateaux, monoclines, fault-scarps, cañons—these things are not peculiar, but only peculiarly striking manifestations of principles with which geologists have to reckon; and British geologists especially will not fail to find in such features valuable clues to the problems offered by the history of their own island.

ALFRED HARKER.

VI.

Sigillaria and Stigmaria.

I.

THE short notice of M. Grand' Eury's admirable memoir on the Geology and Palæontology of the Coal Basin of the Gard,[1] to a copy of which I am so fortunate as to have access, raises some questions which the older workers on *Sigillaria* and *Stigmaria* had supposed to be settled, and on which it may be useful to make a few definite statements.

1. It is quite certain that *Stigmariæ* are not "rhizomes which floated in water or spread themselves out on the surface of mud." Whether rhizomes or not, they grew in the soil or in the upper layers of peaty deposits since changed into coal. The late Richard Brown and the writer have shown that they grew in the underclays or fossil soils, and that their rootlets radiated in these soils in all directions.[2] In one of my papers I have figured a Stigmarian root penetrating through an erect *Sigillaria*, and Logan in his Report of 1845 had already figured a similar example. The penetration of decaying stems by the rootlets of *Stigmaria* is a fact well known to all who have studied slices of Carboniferous plants,[3] while *Stigmariæ* are often found creeping inside the bark of erect and prostrate trunks. Besides this, as I have shown in "Acadian Geology," in the section of 5,000 feet of Coal-measures at the South Joggins (including seventy-six distinct coal groups, and a larger number of soils with *Stigmaria* or erect trees) *Sigillaria* and *Stigmaria* occur together, and the latter nearly always either in argillaceous soils or sands hardened into "Gannister," which are often filled with roots and rootlets, or on the surfaces of coal-beds. On the other hand, the numerous bituminous limestones, and calcareous and other shales holding remains of fishes, crustaceans, and bivalve shells, do not contain *Stigmaria in situ*—the only exception being two beds of bituminous limestone, the upper parts of which have been converted into underclays. This section, and that of North Sydney—two of the most complete and instructive in the world—have afforded conclusive proof of this mode of growth of *Sigillaria* and *Stigmaria*.

[1] NATURAL SCIENCE, March, 1892.

[2] *Quart. Journ. Geol. Soc.*, vol. ii., p. 394 (1846); *Ibid.*, vol. iv., p. 47 (1847); *Ibid.*, vol. v., p. 355 (1849); *Ibid.*, vol. v., pp. 23, 30.

[3] Williamson has noticed this in his excellent memoirs in the *Phil. Trans.*

2. Stigmarian roots are not confined to *Sigillaria*. Brown has figured and described the Stigmarian roots of a *Lepidodendron*.[4] His specimen, it is true, was not very characteristic in so far as the stem is concerned, resembling rather the fossils known as *Lyginodendron*, but I subsequently found at Horton Bluff the Stigmarian roots of *Lepidodendron corrugatum*, a lower Carboniferous species very near to *L. veltheimianum*. This is described and figured in my report on the Lower Carboniferous Flora of Canada.[5]

3. The bulb-like appearance figured by Grand' Eury at the base of *Sigillaria* is, in my experience, an exceptional phenomenon. Of more than a hundred erect *Sigillariæ* which I have examined or extracted, it occurred in only two or three, the others having trunks spreading at the base in the manner of ordinary trees.[6] It may be a specific or varietal character, or a result of the expansion of the lower part of the stem above the line of insertion in the soil and attachment of the roots, just as an exogenous stem becomes expanded above a ligature; or perhaps in some cases it may be an effect of lateral squeezing applied to the more compressible matter, partly organic and partly argillaceous, often filling the lower part of the stem. Erect Calamites are in this way sometimes made to assume a beaded appearance by compression between the nodes.[7]

4. The use made by Grand' Eury of the terms *Syringodendron* and *Stigmariopsis* requires some explanation. The appearance compared to *Syringodendron* at the base of *Sigillariæ* has really no connection with the genus *Syringodendron* as described by Sternberg and Brongniart. This genus includes a peculiar type of Sigillaroid tree, but the base of any ribbed *Sigillaria* assumes the appearance of having double rows of round marks or punctures, owing to the obliteration of the leaf-bases and separation of the vascular marks caused by expansion of the bark. Examples of this will be found in the Quarterly Journal of the Geological Society, vol. x., p. 32, figs. 9, 10, 11; and in "Acadian Geology," p. 180, fig. 30.

The term *Stigmariopsis*, on the other hand, might be fairly applied to those *Stigmariæ* which, like the roots of my *Sigillaria* (*Leioderma*) *sydnensis*, have a wrinkled surface and somewhat irregular areoles.[8] But an appearance similar to this generally occurs on main roots of *Sigillaria*, near to the stem, and arises from the same kind of expansion already referred to as occurring at the base of the trunk. This is, indeed, the explanation deducible from Grand' Eury's description and figures.

5. I am not aware of any instance of a stem of *Sigillaria* springing as a bud from a branch of *Stigmaria* or its termination. There are abundance of examples (quite numerous in my collection) of the

[4] *Quart. Journ. Geol. Soc.*, vol. iv., p. 46 (1847).

[5] Geol. Survey of Canada, 1873. [6] "Acadian Geology," p. 192, figs. 1, 4 (1855).

[7] Figured in "Acadian Geology," p. 198. [8] "Acadian Geology," pp. 435, 475.

conical tap roots springing from the lower sides, but nothing rising from the top, except where a *Sigillaria* stem crowns the meeting of its four or eight main roots.

6. The objection to calling the *Stigmariæ* roots and their processes rootlets, appears to me a finical application of modern botanical usages to times for which they do not hold. We might equally object to the application of the term roots to those which spring from the earthed-up stems of Calamites, radiating as they do from nodes which, in the air, would produce branchlets. Grand' Eury's figures show abundant instances of this. We might also object to the exogenous stems described by Williamson, which belong to cryptogamous plants, and, unlike anything modern, are made up exclusively of scalariform tissue. If the articulation and regular arrangement of those gigantic root-hairs, the rootlets or leaves of *Stigmaria*, are to be regarded as depriving them of the name which clearly describes their function, we may call them underground branches, though, by so doing, we set at nought both their function and their mode of growth.

7. The above remarks show, so far as they go, how completely the doctrine of *Stigmaria*-underclays and the terrestrial growth of coal, as originally explained by Sir William Logan, are borne out by the facts; and I may refer, in further illustration of this, to the remarkable history of the erect *Sigillariæ*, containing remains of land animals at the South Joggins, as given in my memoir on erect trees containing animal remains.[9]

In making the above statements, I would not be understood as derogating in the least from the merits of my esteemed confrère, M. Grand' Eury. His work on the Carboniferous Flora of the Department of the Loire, and the later work above referred to, are triumphs of that careful and elaborate exploration of fossil plants *in situ* which, while most laborious and difficult, gives us the only certain means of restoring, in their integrity, the plants of the old coal forests. No labourer in this arduous field deserves greater credit than Grand' Eury; but, to obtain their full benefit, the results of his labours require to be compared with those of others in different localities, and to be interpreted, not on the limited rules of modern botanical nomenclature, but in accordance with wide views of the versatile methods of nature, more especially in the older periods of the earth's history.

J. William Dawson.

[9] *Phil. Trans.*, 1882, pp. 621–659. I may also refer to a paper by Wild in the last number of the *Trans. Geol. Soc., Manchester*, vol. xxiv., pt. 13 (1892).

II.

The publication of a new work (Géologie et Paléontologie du Bassin Houiller du Gard, 1890) by my old friend M. C. Grand' Eury has again raised the question of the nature of the *Stigmaria ficoides*. My own views on the subject, based wholly upon our British specimens, were embodied in full detail in my Monograph published in 1887 by the Palæontographical Society, and nothing has occurred since its appearance to require me to alter a single statement of any importance which that memoir contains. In it I confined myself, as I have said, wholly to the study of British examples of the plant, paying no attention to the figures and descriptions of Continental specimens which I had no opportunity of studying personally. Hence, respecting such specimens I gave no opinion.

M. Grand' Eury has not, either in his new figures or his descriptions, entered into much detail, but he has reiterated in all its decisiveness the French hypothesis that the *true Stigmariæ* are not roots, but rhizomes, "qui, ayant été incapables de se soutenir, ont flotté dans l'eau ou rampe sur la vase qu'ils ont aussi pénetrée" (p. 236). "Une fois seulement je les ai surpris divergeant d'un centre sous tige." We have here a statement altogether inapplicable to anything that we discover in Great Britain. His hypothesis appears to be identical with the vague and speculative guesses that were prevalent among us in the early years of the present century, and to which the discoveries of Sir William Logan put an end.

Whenever we discover a perfect example of our British *Stigmaria*, like my magnificent one from Clayton now in the museum of the Owens College at Manchester, we invariably find it to consist of a circle of dichotomously-branching roots radiating from a centre whence ascends a stem either of a *Sigillaria* or a *Lepidodendron;* and the recent discovery of several similar, though smaller, examples at Osnabrück in Germany, chronicled by M. Potonie,[1] are exactly of the same character. M. Grand' Eury divides the objects which he discovers in France into two groups. To one of these groups he gives the name of *Stigmaria*, and this he considers to be something very different from our roots of *Sigillaria* and *Lepidodendron*, to which we apply the same name. He describes his Stigmariæ as long rhizomes, not varying much in diameter, or radiating from a common centre. Once only he appears to have met with a specimen which did so radiate. The appendages to these "rhizomes," unless I misunderstand him, which appear to me to differ in no respect from the *rootlets* of our British examples, he and M. Renault regard as leaves. On the other hand, he finds

[1] "Jahrbuch der königl. preuss. geologischen Landesanstalt," 1889.

examples which, so far as I understand his views (which I must confess I am unable to do very distinctly), seem to be identical with our British type, but to which he applies his name of *Stigmariopsis*.

Now, to all this I entirely object. Our British forms are those which have been recognised as the true *Stigmaria ficoides* ever since the days of Sternberg and Artis; and I contend that if our French friends have discovered in their coalfields a new type, it is not for them to divert our old name to their new forms, but to invent for them a new name, *Stigmariopsis* or any other, and leave us in the possession of our own time-honoured title.

Notwithstanding all that has been written by MM. Renault and Grand' Eury on this subject, and which I have studied with careful attention, I am still unable to understand clearly what their views are, beyond their conclusion that some of these objects are rhizomes furnished with leaves, while others are true roots. When I try to comprehend which are the actual examples of the one and the other, I fail to do so. Anyhow, I claim for *all* our British forms, *first*, that they represent the true *Stigmaria ficoides* of the many authors who have written on the subject, and, *second*, that these forms are *roots* furnished with rootlets, and not rhizomes bearing buds and leaves. Imprimis, they are *roots* employed to fix and sustain a noble aerial stem in the ground, and also to extract from that ground such nitrogenous and mineral substances as the giant aerial Lycopodiaceous structures require for their nutrition.

My belief that the appendages which clothe these roots are rootlets and not leaves, is shown by their organisation. Like the roots of recent Lycopods, each one has a single remarkable *monarch* vascular bundle, which M. Van Tieghem first showed to be so characteristic of the roots of the Lycopodiaceæ. Then they possessed that remarkable root-like power and habit of distributing themselves, penetrating every minute aperture into which they could, by the faintest possibility, obtain an entrance, in search of nutriment, a faculty which is wholly foreign to leaves of every kind and degree; besides this, the fact of their subterranean position altogether unfits them for performing any foliar functions. The aerial stems and branches were magnificently supplied with real foliar organs, to which the performance of these subaerial functions was committed. Organs so different in structure and disposition could not possibly be merely modified homologues of an identical organ. Then we have the cardinal distinction, so fundamentally characteristic of almost the entire vegetable kingdom, that these roots are geotropic organs, while the leaves belong to a heliotropic system.

A further proof that the appendages to *Stigmaria* cannot be leaves is shown by the *origin* of their vascular bundles. Those of the true aerial leaves spring solely from the primary, or non-exogenous xylem, which terminates at the base of the aerial stem, and never enters the roots. The vascular cylinder of the latter organs, on the

other hand, consists of the secondary or exogenous xylem, and it is from this structure that the monarch bundles of the rootlets are derived. Two such absolutely different zones of the wood could not supply homologous organs.

I repeat, therefore, my conviction that our true British examples of *Stigmaria* are, and can be none other than, true roots; and, so far as my limited acquaintance with the German specimens enables me to form an opinion, they appear to belong to our British type. That some different objects may be discovered in the French collieries, I am alike unable to affirm or to deny; but I am compelled to entertain doubts on the subject from more reasons than one. I am unable to reconcile the descriptions of M. Renault with those of Grand' Eury. I cannot obtain any clear and definite account of the nature of the bulbs supposed to be developed at the under surface of the so-called rhizomes. In my Monograph (*loc. cit.*, fig. 18) I have described roots of our *Stigmariæ* from a specimen which cannot have been much more than one-fifth of an inch in diameter, to the large Clayton example in the Owens College Museum, which extends nearly thirty feet from side to side; yet in none of these do we discover the faintest trace of any bud or bulb, nor yet of any indication of a point to which such a growth could have been attached. The idea of the possibility of some gigantic protonemal condition having been the first product of the germinating Lepidodendroid spore has for some time past suggested itself to my mind, such a protonemal state being represented somehow by the "Rhizomes" and "Bulbs" of which the French authors speak; but, until we obtain more exact details and figures of these French structures than we at present possess, I must confess to a large measure of doubt being left on my mind in reference to the entire subject of these supposed discoveries and the conclusions based upon them.

Further, when Grand' Eury is describing what appear to me to be the French representatives of our British *Stigmariæ*, he defines their roots as "court." One of these roots, which I measured at Clayton, was 37 ft. 4 ins. long, and while at its thick base the root was more than 8 ft. in circumference, it tapered away at its apex to a mere point.

My valued friend tells me that there is really less difference between his views and mine than I believe to exist, and the acquisition of more exact details and clear definitions than we have yet got may prove that this is the case. He is too indefatigable and distinguished a geologist, as well as too conscientious a man, to make it possible to doubt that he is in the possession of some facts which have no parallels in Great Britain. How far what at present appears to be his interpretation of those facts will prove correct, yet remains to be ascertained. Personally, I am yet as incapable of understanding them as of rating them at their true value.

W. C. WILLIAMSON.

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Fig. 3.—The Southern Walls of Chimborazo: under Point Veintemilla. B. Upper Cliffs; D. Lower Cliffs; E. Vertical Section of the Glacier on the Summit; F. Possibly the point reached by Humboldt and Boussingault; X. A Lichen found here.

SOME NEW BOOKS.

TRAVELS AMONGST THE GREAT ANDES OF THE EQUATOR. By Edward Whymper. With "Supplementary Appendix" by H. W. Bates, T. G. Bonney, G. A. Boulenger, &c. 2 vols. 8vo. London: J. Murray, 1892. (Vol. ii., 1891.)

IT is now nearly twelve years since Mr. Whymper returned from his expedition among the Andes of Ecuador, and probably the first impression upon receiving the two handsome volumes, in which he records his experiences and discoveries, will generally be one of irritation at having had to wait so long; and this feeling is somewhat excusable, for Mr. Whymper unquestionably ranks as the most popular English writer on mountaineering. Other climbers may have led the way up more difficult peaks, have added more to our knowledge of the geology of the higher ranges, or have possessed a sounder grasp of the broad outlines of Alpine topography; but Mr. Whymper's connection with one of the most famous of Swiss mountains, his clear and easy English, his racy humour, and his exquisite wood engravings, have roused an interest in his achievements among a much larger circle of readers than usually follows the progress of mountain exploration. As soon as we get into the book, however, we find that the time has not been wasted, for every page gives evidence of the conscientious care and elaboration which is typical of Mr. Whymper and all his works. He did not go to South America simply to "do" Chimborazo. Mr. Whymper treats his mountains with more respect, and prefers to carry them by a regular campaign rather than a sudden assault. Moreover, besides ordinary exploration, he wished to ascertain the results of a long residence at high elevations, and to test the accuracy of the aneroid as compared with the mercurial barometer. For these purposes he would have preferred a higher region, but political troubles then rendered both the Himalaya and the Southern Andes unsuitable for experiment, and so he had to put up with Ecuador. The preparations were most elaborate, and occupied as long a time as the journey itself. Perhaps the only point one feels inclined to question was the wisdom of the selection of J. A. Carrel as chief guide. The mountaineering seems, as a rule, to have been quite easy, and to have afforded few opportunities for the display of Carrel's prowess as a rock climber; there were difficult rocks such as the aiguilles on Chimborazo (Fig. 1)[1] but they never came in the way, and for most of the time the guides were only employed as porters, or in other still less congenial work, which could have been equally well done by a man less generously endowed with self-will than the famous J. A. Carrel.

Mr. Whymper and his guides landed in Guayaquil on the 9th

[1] These figures have been reproduced by the kind permission of Mr. Whymper, but they are inadequate representations of the originals.

December, 1879, and he remained in Ecuador till the end of the following July. He first made for Chimborazo, the highest peak in this part of the Andes, and one which had never previously been scaled. This was successfully ascended on the 4th January; the summit was reached from a camp, the highest of three, placed at an elevation of 17,285 ft. At the second camp, both Mr. Whymper and the Carrels were prostrated by mountain sickness in a very acute form; this rendered it necessary to proceed slowly so as to become gradually acclimatised to the low atmospheric pressure. Mr. Whymper proposed to camp on the summit, but the Carrels struck work and declined to reascend, for Louis Carrel, having failed to provide himself with gaiters, had had his feet frost-bitten; as it was their own fault, the guides concealed this fact as long possible. Mr. Whymper was also taken ill, so that climbing had to be temporarily abandoned, and the party retired to Ambato to be doctored. On recovery, they went north to Quito, ascending Corazon (15,871 ft.), and Cotopaxi (19,613 ft.) on the way. They camped for a night close below

FIG. 1.—Aiguilles on Chimborazo.

the summit of the latter, and Mr. Whymper draws a most graphic picture of the interior of the crater at night. Just as a treat to the guides, as a change from the "severely scientific" ascent of Cotopaxi, Mr. Whymper turned aside and climbed the sharp peak of Sincholagua: it was the first ascent and it will probably be the last for a good long time. After passing through Quito, Mr. Whymper made the first ascents of Antisana and Cayambe, the third and fourth peaks in height in the Northern Andes. Disappointed in the eastward view from Cayambe, he crossed the watershed, and after a miserable week of perpetual rain in a district that was as wet as a saturated sponge, he climbed Saraurcu; but the mists again robbed him of the cherished view into the basin of the Upper Amazons. The quest for antiquities then led Mr. Whymper into the extreme north of Ecuador, where he found old pottery, and stone implements, and the way up Cotocachi (16,301 ft.). On the return journey, he reascended

Chimborazo, witnessing on the way a violent explosive eruption of Cotopaxi, which in less than a minute shot a cloud of ash to a height of 20,000 ft.; the column was then deflected at right angles by a strong upper current, and spreading out as a cloud made the summit of Chimborazo dark at midday (Fig. 2).

Those who turn to Mr. Whymper's book in the hope of finding stirring stories of difficult climbing will be disappointed; Mr. Whymper's narrow escape by a fall through an *ice*-covered crevasse is almost the only thing worth calling an adventure. Bad weather gave them serious trouble and at times rendered the camp "as comfortable as a wet ditch on a winter's night," while an inn was discovered at Ambato with more fleas per square yard than Mr. Whymper had previously met with, so the record of the Alpine hay chalets is broken at last. But the volumes make up in careful observation and scientific results for any lack of sensational incident. Mr. Whymper has shown that instead of the orthodox two ranges of Cordilleras, the western one rising steadily from the sea, the main chain of the Andes consists of a rather irregularly scattered series of peaks and basins, while an important

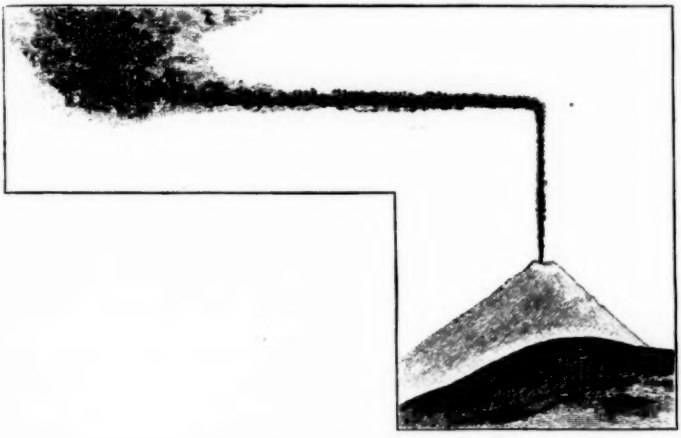

FIG. 2.—Commencement of the Eruption of Cotopaxi.

series, named by Mr. Whymper the "Coast Range," occurs between the Andes and the Pacific; the mountains of the district are all of volcanic origin, except Saraurcu, the easternmost peak reached, which is composed of crystalline schists, referred by Professor Bonney to the Upper Archæan; the rest are of andesites. Only Cotopaxi and Sangai are permanently in eruption, and in the others the craters are probably buried in snow and ice. Mr. Whymper has also shown that the higher mountains are glacier-covered, and found traces in places of the former greater extension of the glaciers. Clear evidences of a stone age have also been discovered.

One of Mr. Whymper's main objects was, as we have said, to determine the exact influence of the low pressures of high altitudes upon men, and whether it would be possible gradually to become acclimatised to them sufficiently for it to be possible to scale the highest of the Himalaya. Mr. Whymper shows fairly conclusively

that low pressure has a distinctly weakening effect, even after a six months' residence, but that the acute form of mountain sickness is only temporary; hence there is good reason to hope that by rising by slow and easy stages the very highest summits on the earth may be reached. One of the most valuable contributions made to geographical science by the expedition was the comparison of the aneroid with the mercurial barometer; two of the latter were carried, and a whole battery of aneroids. Distrust of the aneroid has often been expressed, and Mr. Whymper shows with what good reason. The main record of this work has been given in a separate pamphlet, but such constant reference to it is given that the main conclusions may be referred to. The aneroid appears to be perfectly reliable for *sudden* differences of pressure, but in a long and gradual ascent the aneroid drops considerably below the right point, and continues to drop after it has been brought to rest. This error amounted on the summit of Chimborazo to 1·152 in., and recorded the summit as 1,500 feet too high. By a prolonged series of experiments, after his return, Mr. Whymper maintains that he has found out the cause of this error and how it can be allowed for, so that implicit faith can in future be placed on aneroid measurements. We very, *very* much wish we could believe it.

Mr. Whymper's zoological collections were very extensive, and part of them are described by various specialists in the second volume. Mr. Whymper is heartily to be congratulated on having devoted his attention to the lower vertebrates and invertebrates, instead of spending all his time in trying to catch a puma two inches above record length, or a lama with two tails. The special interest of the collection is in regard to its bearing on the theory that the fauna of the higher Andes is a relic of a temperate fauna, and worked its way across the tropics during the glacial period. The collections made by Mr. Whymper show that the high fauna is simply composed of representatives of the genera of the adjoining tropical lowlands; the genera characteristic of the temperate regions both north and south do not occur in the area, and a pretty theory of a faunistic migration falls to the ground.

But probably the most interesting results of Mr. Whymper's studies are what he has taught us to unlearn. Ecuador was once a wonderful place, and contained wonderful animals whose achievements were vouched for by eminent authorities; but Mr. Whymper has mercilessly unmasked several of the most startling of these marvels. Thus, the grand old Condor which, in the words of Professor Orton, "can dart in an instant from the dome of Chimborazo to the sultry coast of the Pacific," and which Humboldt saw soaring over the loftiest summits of the Andes, turns out to be a sorry fraud. Mr. Whymper's observations show that at present the Condor is restricted in Ecuador to a range of from 9,000 to 16,000 ft. Humboldt's Condors may, perhaps, have been frightened up by one of the revolutions to which they have now grown accustomed; and, until Professor Orton tells us how his bird was identified at the various stations it passed at the fine pace of over 400,000 miles an hour, perhaps he will excuse our accepting his testimony as worth anything, although he be a Professor of Natural History in New York, and was sent out by the Smithsonian Institution.

Serious doubt is thrown by Mr. Whymper upon the whole of Humboldt's story of his exploits on Chimborazo, with his ascent to the height of over 19,000 ft. and his marvellous descent of 3,600 ft. along

a difficult arrete in an hour, a rate which the astounded Whymper declares to be "simply divine;" but, as Humboldt always spoke of the summit as single instead of double, and as he said that there were no glaciers upon the mountain, whereas the whole summit is clad with them, and it is impossible to reach anything like the height Humboldt claimed to have done without crossing a good lot of glacier, it seems difficult to know what weight should be attached to his account. But before we dismiss it as an entire fiction we may remember that in much later times Professor Whitney and a party of American geologists succeeded in surveying Mount Shasta, and reporting a complete absence of glaciers, whereas all subsequent visitors have found several.

Another story that has long done duty in our text-books, on Humboldt's authority, is that of the subterranean fish erupted from Cotopaxi alive and dead, and in such vast numbers that, if his account had been accepted literally, geologists would have included fried fish among the normal volcanic ejectamenta. But after Mr. Whymper's *exposé*, *Cyclopium cyclopum* may join the resurrected rotifers as the latest addition to the rank of zoological fables.

The general results of Mr. Whymper's expedition are certainly of very high value; he has cleared the ground of many serious blunders, and given us a good basis of solid fact and accurate observation which must prove of permanent service in further explorations of the geography, geology, and zoology of the Ecuadorian Andes. Mr. Whymper has obviously been to enormous trouble, both in the field and at home; but we hope he feels that the labour has not been excessive, for his scientific results are of great value, and he has issued them in a work so well illustrated, bound, and printed, that it will at once take its place as one of the choicest classics in the literature of mountaineering.

J. W. G.

THE LABRADOR COAST: A Journal of Two Summer Cruises to that Region. With Notes on its Early Discovery, on the Eskimo, on its Physical Geography, Geology, and Natural History. By A. S. Packard. 8vo. Pp. 513. With Maps and Illustrations. New York: N. D. C. Hodges; London: Kegan Paul, Trench, Trübner & Co., 1891. Price 18s.

SINCE in these days of specialism it is but seldom that an author is qualified to write of a comparatively unknown region alike from the geographical, geological, and zoological points of view, Dr. Packard's work ought to appeal to a wider circle of readers than the majority of books of scientific travel. And so well has the author accomplished his task, that the work remaining for future investigators of the Labrador Coast will be mainly confined to filling in the details of Dr. Packard's masterly sketch. The interior of the country, however, still offers a wide field of investigation to such hardy explorers as care to brave an inclement climate, a rugged country, and the attacks of the swarms of black-flies and mosquitoes which made life at times so unbearable to Dr. Packard's party.

It speaks well for the accuracy of the author's observations that a large part of the work relates to cruises made on the coast as far back as the years 1860 and 1864, and that his descriptions of the geological and zoological results of these two cruises are reprinted, with but little addition and alteration, from the Memoirs of the Boston Natural History Society for 1867. Other portions of

the work, including the account of the author's first cruise to Southern Labrador in 1860, and the first chapter of the voyage to the northern part of the country in 1864, have also seen the light in various serials; but it appears that the other four chapters descriptive of the latter voyage, as well as one on Recent Explorations, are now published for the first time.

The earlier portion of the work is devoted to a description of the physical geography of the country, and to an investigation into the history of its discovery, from which we learn that Labrador was known to the Norsemen "nearly five centuries before John Cabot made his first landfall of the American Continent." The chapters descriptive of the author's two journeys to Labrador contain much entertaining reading, and give a good idea of the general characters and productions of the country. Many interesting observations are recorded on the sea-birds of the coast, but we wish that the author was not quite so fond of calling one and the same bird by different names, since this is sometimes rather puzzling to his readers, as, for instance, on page 103. Then, again, although we are well aware that the puffin is frequently known as the "sea-parrot," we must object to its appearance under the name of "perroquet," without any qualifying prefix (p. 66). The vast diminution in the number of the gannets referred to on page 98 shows how urgent is the need of protection for sea-birds, even in such a remote region as Labrador; this necessity being even more apparent in the case of the eider-duck, which was threatened with extermination until the forays of the "eggers" were stopped.

While the zoology and botany are fully treated, the ethnology and geology of the country receive full attention from the author, and the experiences during his own voyages are well supplemented by a short chapter on recent explorations. Ample lists of the fauna and flora of the coast are given in the concluding chapters of the work before us, which will probably long remain the standard authority on all matters connected with the "Land of Labour."

PHASES OF ANIMAL LIFE, PAST AND PRESENT. By R. Lydekker. 12mo. Pp. 248. London: Longmans, Green & Co., 1892. Price 6s.

THIS little volume, which is the first of the "Knowledge" series, is a reprint, with some additions, of articles originally appearing in our contemporary *Knowledge*. Needless to say, the whole of the subjects are treated in a popular manner, as the work is intended solely for those having an interest in natural history, but not professed students thereof. There are many somewhat similar works devoted only to the animals of the present epoch, but the especial claim to attention of the one before us is that it endeavours to illustrate, in a popular manner, some of the leading features of many extinct groups of vertebrates. It is, indeed, almost exclusively with vertebrates that the work deals, but it must by no means be inferred that fossil forms alone are referred to. There is a chapter on Tortoises and Turtles, and there are others on Marsupials and Monotremes, all of which deal mainly with living types. Some of the chapters treat solely of the structural features of various groups, living or extinct, while others are designed to show how different groups of animals are modified for similar modes of existence; and others, again, show the modifications which particular organs, like teeth, have undergone in different orders of animals. The volume is illustrated by nearly

eighty figures, of which the great majority have previously appeared in other publications. A striking feature in the work is the small extent to which technical terms are employed, even when structures of considerable complexity are treated; and this absence, together with the general interest of the subjects, ought to ensure for it an extensive circulation among that section of the public for whom it is primarily intended.

TABLE AND MARKET POULTRY VERSUS FANCY FOWLS. By W. B. Tegetmeier. 8vo. Pp. 111. London: Horace Cox, *Field* Office, 1892.

IN the little work which bears the above title, written though it is with a pre-eminently practical object, Mr. Tegetmeier has made a most interesting contribution to Zoology. During the time that has elapsed since he published his well-known work on Poultry, the fanciers have not stood still; but the result of their labours in selective breeding has been, as Mr. Tegetmeier conclusively proves, a continuous degradation of the breeds they have taken in hand, regarded as profitable stock.

This is very strongly insisted upon in the introductory chapter of the work, and attention is drawn to the similarly bad effects of competitive shows on the practical qualities of other animals, such as dogs and pigs. To show what the farm-yard fowl was before corrupted by admixture with various artificial breeds, we are presented with a reproduction of Bewick's figure, representing "a short-legged, strong-winged, full-chested bird of great value as a table-fowl—one that is in strong contrast to the coarse-boned, narrow-chested, feather-legged mongrels" that have been the result of crossing with the Cochin and Brahma.

The next nine chapters deal with the several breeds separately or in groups, the game fowl, in which the change has been especially conspicuous, being taken first. This unfortunate variety has been, in the pursuit of what is known as "reachiness," elongated out of all knowledge, so that it reminds one of a wading bird, hens having sometimes the keel of the breast-bone raised, when in the standing position, 12 inches off the ground. The practice of cock-fighting, however morally reprehensible, certainly tended to the production, by survival of the fittest, of compact, vigorous, and meaty birds, superior in every way to their present representatives; and, to show the contrast, the frontispiece gives portraits of a young game-cock of the modern type, and one of the old fighting strain, represented as trimmed and "heeled" for the pit, its more full-bodied character being thus rendered apparent. It may be of interest to note here that a recent writer on the old fighting game, Mr. Fletcher Moss, states that in the examination of twenty-nine fighting specimens he found that the general plumage was either the black-red (the colour of *Gallus bankiva*) or some approximation to it. As the "cockers" cared nothing for colour, this must mean either that the absence of selection for this point allowed of reversion, or that the natural coloration was correlated with a stronger constitution and a higher courage, though it must be noted that in the celebrated Lord Derby strain the legs were white, unlike those of the wild bird.

Dorkings are next treated—a breed whose original usefulness as table-fowl has been sadly impaired by the encouragement of coarse-boned specimens, the defect having originated through a cross with the Malay, resorted to in order to gain increase of size.

There is an interesting statement by Mr. John Baily to the effect that white fowls have a tendency to yellowness in the fat. It may be added that fowls of this hue are apt to be tanned or discoloured by the sun.

It is gratifying to notice a protest against the fifth toe, an additional hallux, so characteristic of this breed. Easily perpetuated, it is "an unquestionable deformity," and connected with disease of the foot.

With regard to Cochins, a breed which Mr. Tegetmeier is inclined to think might have had an independent origin, they are stated to have given the first strong impulse to the now prevalent pursuit of poultry-keeping. A characteristic illustration allows us to judge of the alteration wrought in the breed in the few decades which have elapsed since its first exhibition in 1850; and progress is easily seen to have been mainly in the direction of the production of a mass of useless feather, especially on the shanks, where it is most objectionable. On the other hand, the main economic fault of the breed, its deficiency of flesh on the breast, correlated with the extremely weak powers of flight, has not been corrected. It is very properly pointed out that the extra feathering is merely so much food wasted.

Brahmas, which for practical purposes are stated to be identical with Cochins, are likewise portrayed as they were and in the "improved" form of to-day. Mr. Tegetmeier regards these as a manufactured breed. Mr. Lewis Wright has mentioned several interesting points wherein they tend to resemble *Gallus furcatus*, though not assigning them a direct descent from that species; the single median wattle having been found in at least one instance, and the non-serrated comb, and greenish gloss on the plumage, having also occurred in this variety. But such resemblances as these may be doubtless set down to analogical variation; just as, in the domestic duck, about whose descent from the mallard there is no doubt, the white on the tertiaries, characteristic of the Spotted Bill (*Anas poecilorhyncha*) may be occasionally observed.

It is curious that with Brahmas, as with several other varieties, it is necessary to maintain two distinct breeding studs for show birds, since equally perfect specimens of both sexes cannot be bred by mating a cock and hen which satisfy exhibition requirements. This, of course, does not increase the usefulness of the varieties in question.

Other Asiatic breeds of more recent introduction are next dealt with. The Langstan, at first hardly differentiated from the Black Cochin, has been bred larger-winged and closer-feathered than that bird, besides having less feathering on the legs; yet, as a table fowl, it appears not to be of high quality.

Plymouth Rocks are a good example of a new breed, produced by crossing older varieties, a practice which the present system tends to encourage. A new variety, it appears, can be bred tolerably true to form and colour in three or four generations, and is then likely to become the rage if sufficiently advertised. This has been the case with the recently-invented Wyandotte breed, whose distinct markings, unfortunately, seem to be leading its adherents away from the cultivation of its useful qualities. This, like most of the leading breeds, has its club of admirers.

Under the head of Malays, Indian game, and Aseels, are included breeds of Eastern origin, characterised by very scanty combs and

wattles, close plumage, and often a very savage disposition. The Malay is too long in the leg to be of much practical value, but the nearly allied breed, miscalled Indian game, is an excellent table-fowl. The true fighting-cocks of India belong to the Aseel breed, the chief drawback to which is its extreme pugnacity. The young cocks, we are told, "will fight while little chickens, and when they can no longer stand will lie upon the ground and peck at one another." The small combs of these very pugilistic birds do not afford much support to Eimer's view, that frequent stimulation by seizure in fighting has led to the increased size of the comb in domestic poultry as compared with the wild birds.

Even the French table-breeds, which come next in order, have not escaped deterioration; the modern Houdan, with its head like a feather mop, presenting a woeful contrast to the moderately crested bird at first imported. These first importations, too, were often without the additional toe; but this useless deformity has now been established as a feature of the breed. In this chapter, too, attention is drawn to the curious fact that "large combs, although they show that the birds possessing them are good layers, also indicate that they are not readily fattened"; though in the case of breeds where a large comb is a show-point, it would not be safe to select the largest-combed hens as layers. This is the case with the various breeds of Mediterranean origin, to which a chapter is devoted. The best known is the Spanish, whose chief point, for many years, has been a perfectly white face and ear-lobe. This latter appendage has been so developed by cultivation as to become 3 or 4 inches in length, and in pursuit of this and similar excellences the breed has been rendered extremely delicate and infertile; although a writer in the *Stockkeeper* maintains that Mr. Tegetmeier has overstated the case against the modern specimens of this formerly profitable breed, saying that the prize hen alluded to as barren had really laid one egg, while, as to hardihood, a high-class cock, kept in an open shed during the winter, had only lost part of his comb and the ends of his toes. But even such prolificacy and hardihood as this can hardly be considered sufficient for practical purposes. These Mediterranean varieties do not sit, and this absence of the instinct for incubation, so advantageous for egg-production, is also characteristic of the Hamburgs and Polish, treated, with other varieties, in Chapter X. The former of these, though very beautiful, are, like the latter, whose heavy crests have always been a drawback to their usefulness, not to be recommended for practical purposes.

In the excellent chapters on the practical management of fowls, we are glad to see that the notion of crooked breasts arising from the use of small perches is contradicted. There is a too prevalent idea among bird-keepers that round perches are objectionable, even for passerine birds, whose perfectly constructed grasping feet are so eminently suited for holding a round support. With regard to hatching, it is stated that eggs with any irregularity of shell should invariably be rejected, as they will not hatch. In France, chickens are largely hatched by turkey-hens, and it might have been mentioned that the cock-turkey will also undertake this task.

With regard to ducks, the Aylesbury is the breed recommended. That neglected species, the Muscovy (*Cairina moschata*), is not mentioned, though its hybrids with the common breeds must possess some value, since a very practical breeder advertises eggs from the Aylesbury, mated with this bird, for sale, and it is not in repute as a

fancy bird. We hear nothing, too, of a breed of ducks which has recently come into notice under the mysterious name of Indian Runners, which are said to be gifted with extraordinary laying powers. As to geese, attention is drawn to the fact that they maintain their productiveness for an unusually long period, up to even twenty years; and with turkeys it is noteworthy that much additional vigour has been gained by an admixture of American blood, the American breed having been largely crossed with the northern race, *Meleagris gallo-pavo*, so that, supposing the domestic birds to have arisen from the southern form, *mexicana*, we now have two subspecies, at least, united in some of our tame turkeys.

In the chapter on diseases, an interesting account, with an illustration of the bacillus, is given of a disease which proved very fatal on a poultry farm in Kent in 1888. The folly and impracticability of these ventures is fully pointed out in the succeeding chapter, but with this we have not at present to do.

Far too little attention is usually paid by naturalists to the progress of the domestication and breeding of various animals, and deplorable as they are from the practical and æsthetic points of view, there is no gainsaying that the too often monstrous productions known as fancy breeds possess a genuine scientific interest. A debt of gratitude is undoubtedly owing to Mr. Tegetmeier for having so vividly detailed in this work the results of the last 40 years' breeding on the plastic organisation of *Gallus bankiva*.

FRANK FINN.

THE WORLD-WIDE ATLAS OF MODERN GEOGRAPHY, POLITICAL AND PHYSICAL, containing 112 plates. With an Introduction by J. Scott Keltie. Edinburgh and London: W. and A. K. Johnston, 1892. Price 7s. 6d.

THE "World-Wide Atlas," considering the exceedingly low price at which it is published, is a remarkable production, containing in a very small space as much information as can be obtained from many larger and more expensive atlases.

The introduction by Mr. Scott Keltie gives a concise epitome of the present state of our knowledge of the geography of the different regions of the globe, accompanied by notices of the history of the gradual acquisition of this knowledge and the names of the chief explorers.

The maps, which measure 10½ by 7½ inches, are, for clearness and excellency of engraving, admirable examples of the chartographer's art, and appear thoroughly up to date. Not only are the various continents mapped geographically, but a replica of each of the continental maps is given, with the physical features clearly indicated by distinctive colouring so that the general contour can be comprehended at a glance. While the larger countries have each a separate map of their whole area, other maps on a larger scale illustrate in greater detail their provinces, and the Atlas can thus vie with many of its larger rivals.

While not too detailed for the young scholar, the "World-Wide Atlas" will be found sufficiently comprehensive to serve the purpose of many who require the aid of good maps for their special studies.

SOME NEW MEMOIRS.

ON THE PHOTOGRAPHIC ANALYSIS OF PLANT MOVEMENTS. By Dewèvre and E. Bordage. *Revue générale de Botanique*, February 15, 1892.

PHOTOGRAPHY has already been applied with so much success to the study of animal locomotion, that MM. Dewèvre and Bordage have lately been adopting this method for determining the movements of plants. To the top of the part to be studied, a minute thread of blackened glass, ending in a bright head, is attached by a drop of glue. The movement of the bright head is recorded on a sensitised plate contained in a photographic camera. By means of two cameras placed one above, and the other by the side of, the plant, a record of the movement in a horizontal and a vertical plane is obtained at the same time. The plant may be placed in a blackened box, and the tubes of the cameras inserted in holes in the sides, while the glass head is illuminated by light entering at a small hole in one side; but as continued darkness is often disadvantageous and sometimes not allowable from the nature of the experiment, the authors find it sufficient if the mouths of the cameras are directed towards internally-blackened tubes, placed beyond the part the movement of which is to be registered.

Experimenting with the Convolvulus, they have been able to certify that this climber exhibits true heliotropism, a fact denied by several authors, though Darwin thought it existed, but in a very slight degree.

They have also studied the action of light of different colours on the heliotropic movement, *e.g.*, in the case of *Lepidium sativum*, which is not a climber, they found yellow light to have no action. The curvature of the stem in response to illumination began in the green, increased towards the blue and violet, and reached a maximum at the limit of the violet and ultra-violet. It then slowly decreased in the ultra-violet region, but was still feebly manifested even in that part of the spectrum where silver-salts cease to be blackened and fluorescent substances to shine, and only stopped entirely a little further on, at a distance equal to twice the length of the luminous band obtained with a quartz prism.

An attempt was also made to analyse the normal movements of circumnutation described by a root pushing through the soil. As the presence of an indicator, however small, attached to the sensitive tip of a root causes a Darwinian curvature in the opposite direction, another method had to be employed. The whole root was covered with a thin layer of lamp-black, except the tip, at the level of the region of growth. This part remaining white, affected the sensitive plate, provided sufficient light was received. By this arrangement it was determined that the root, thus left quite at liberty, described a very regular circumnutatory movement, with less oscillation than the tip of the stem.

The sleeping and waking movements of leaves of several leguminous plants were also studied. The leaf observed was usually covered with lamp-black, and then a little flour put on the apex, which was thus able to register the movements. To obtain separate records of the repeated upward and downward movements, it was also necessary to place the plant on a slowly revolving plane. By this means the authors find that the apex of the moving leaf describes a series of very regular oscillations. It arrives, by a series of downward and upward movements, at the state of repose on which it enters by a rapid succession of slight tremulous movements. There are always, between the state of repose and the normal, two distinct periods, which the authors term phases of *oscillation* and *tremulation* respectively.

EXPERIMENTAL RESEARCHES ON THE POSSIBLE ACTION OF GAS AT HIGH TEMPERATURES, ACCOMPANIED BY VERY GREAT PRESSURE AND VERY RAPID MOVEMENT, IN VARIOUS GEOLOGICAL PHENOMENA. By A. Daubrée. *Bull. Soc. géol. France* [3], vol. xix., p. 313 (1891).

M. DAUBRÉE, the well-known author of *Géologie Expérimentale*, has turned his attention to the geological action of gas at high temperatures and pressures.

Charges of dynamite and gun-cotton were exploded in a small chamber, the orifice of which was closed by cylinders of rocks of various kinds, such as granite, limestone, basalt, trachyte, and the Pultusk meteorite. The pressures realised in the experiments varied from 1,100 to 2,300 atmospheres, and the temperatures were estimated to range from 2,500° C. to 3,200° C. It was found that rocks of various kinds could be readily perforated, and that the channels by which the gas escaped were striated. Vertical pipes or "necks," such as those in which the diamonds of South Africa occur, are believed to have been formed in this way. The striations on the sides of these pipes, and the absence of contact metamorphism in the surrounding rocks, are easily explained on this hypothesis.

The nature of the powder produced by the pulverisation of the rocks was examined by placing a piece of cardboard coated with vaseline some distance above the orifice. The larger fragments perforated the cardboard, but the minute particles were retained by the vaseline. They were found to consist of angular fragments of the constituents of the rocks, quartz, felspar and mica in the case of granite, and small opaque, or nearly opaque, globules bearing a striking resemblance, at any rate so far as form and size are concerned, to the so-called cosmic spherules of the deep-sea deposits.[1] The author does not deny that the passage of meteorites through the air must be accompanied by the formation of such globules as those described by Messrs. Murray and Renard, but he points out that terrestrial agencies may also be concerned in their production.

One of the most striking results of the experiments is the proof that, in the extremely minute fraction of a second, during which active change is going on, rocks may be moulded to the form of the surface against which they are pressed. This moulding is so perfect that the striæ left by the tool used in turning out the cavity in the mass of steel are reproduced on the rock. Microscopic examination shows that this plasticity is due to the fracture and re-cementation of

[1] See NATURAL SCIENCE, No. 1, p. 21.

the constituents. If any doubt still remained as to the possibility of the most rigid rocks becoming plastic under earth-stresses, it would be removed by these remarkable experiments.

In the concluding part of the paper, the author discusses the bearing of his experiments on the ejection of masses of rock, molten or otherwise, from the interior of the earth through the channels of communication opened by explosive action. He points out that volcanoes are distributed so that in one and the same district there is a general uniformity in the heights of the principal vents. The elevation characteristic of any particular group is, according to him, a measure of the intensity of the gaseous pressure capable of being developed in the subterranean regions, from which the volcanoes of that group derive their energy.

CONTRIBUTIONS TO THE KNOWLEDGE OF THE LIPARITES OF ICELAND. By Helge Bäckström. *Geol. Fören. i Stockholm Förhandl.*, vol. xiii., pp. 637–682 (1891).

THIS is a petrographical description of the acid igneous rocks of Iceland. It is of interest to British geologists because it confirms and extends the conclusion, arrived at by previous observers, that there is a close similarity between the rocks of Iceland and those of the North-east of Ireland and West of Scotland.

Acid igneous rocks are now known from many parts of Iceland. They occur as lava streams of very recent date, and also as dykes, stocks, and sills, intrusive in the basalts. The principal types are classed as liparite, obsidian, pumice, and granophyre. The most striking chemical fact is the very general predominance of soda over potash. Mineralogically, the liparites are composed of felspar, pyroxene, iron-ore, zircon, and glass. Quartz, tridymite, apatite, and olivine have also been observed; and, in a few cases, hornblende, biotite, hypersthene, and titanite. The porphyritic constituents are principally striated felspar and pyroxene. The silica percentage ranges from 63 to 79. In the frequent occurrence of striated felspar and pyroxene among the porphyritic constituents, and in the relative proportions of the alkalies, there is a resemblance between the Icelandic liparites and the pitchstones and felsites of Eigg and Arran.

The granophyres also furnish a most interesting point of resemblance between the British and Icelandic areas. These occur as intrusive rocks in the basalts of both areas. They were discovered *in situ* by Thoroddsen on the north coast of the Snäffels peninsula in West Iceland, and by Helland in three localities in South-east Iceland. In the former locality, plagioclase is the oldest constituent of the rock. Each grain is surrounded by a narrow zone of orthoclase, which in its turn is surrounded by micropegmatite. Ferromagnesian constituents are very rare. They are represented by biotite in this rock; but in a similar rock known only in ejected blocks, and formerly supposed to be a definite mineral (krablite), a green augite occurs.

It seems impossible to avoid the conclusion that the volcanic rocks of Iceland and the West of Scotland have been derived from the same magma-basin, and that the sequence has been the same in both cases.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

We have received the prospectus of "A Summer School of Art and Science," detailing the arrangements for the sixth annual meeting of the "Edinburgh Vacation Courses." The lectures and excursions, which are to be conducted by Professor Geddes, Mr. Arthur Thomson, Professor Mavor, and others, extend from August 1 to August 31. Among those who will lecture upon Biological subjects are, besides the above-mentioned gentlemen, Professor Lloyd Morgan, Professor Haddon, Dr. Haycraft, and possibly Professor Haeckel and M. de Varigny. These courses of lectures correspond to those held at Oxford and Cambridge by University Extension Societies; and as the meeting of the British Association will take place at Edinburgh during the same month, it is probable that a good many persons will be induced to extend their visit so as to attend the entire series. A modest fee of three guineas covers the lecturers' fees, and arrangements are made to board intending students at a moderate cost. Applications should be made to J. Arthur Thomson, Esq., University Hall, Edinburgh.

Dr. D. H. Scott shortly resigns his position as Assistant-Professor of Botany at the Royal College of Science, having undertaken the charge of the Jodrell Laboratory at Kew. Some good work in anatomy or morphology will doubtless result from this release from the trammels of teaching.

The name of M. Vasseur is placed first on the list of candidates nominated by the Academy of Sciences for the vacant Professorship of Geology at the Paris Museum of Natural History.

We hear that Professor H. S. Williams, of Cornell University, N.Y., has accepted a Professorship at Yale, where he acted for several years as assistant to Professor Marsh.

Dr. Heincke, a well-known authority on fishery matters, has been appointed director of the newly-established Imperial German Zoological Station at Heligoland.

Dr. Hugh Robert Mill has been appointed Librarian to the Royal Geographical Society, in succession to Mr. Scott Keltie, who is now Assistant-Secretary. Dr. Mill has made important contributions to Physical Geography.

THE Royal Scottish Geographical Society has established a London branch at No. 20 Hanover Square, W. At the inaugural meeting last month, the Marquis of Lothian presided, and Professor James Bryce delivered an address on "The Migrations of the Races of Men considered historically." The three primary disturbing causes were described as food, war, and labour, which had in their turn led to the transference, diffusion, and permanence of types. There had been a constant tendency towards reduction of the number of varieties of men. Assimilation had been going on in every part of the world, so that to-day along the shores of the Mediterranean there were but nine different languages spoken, whereas in the time of Herodotus there were probably over thirty-seven distinct tongues. At its London meetings the Scottish Society proposes to deal chiefly with commercial geography.

THE Manchester Geographical Society has arranged a course of lectures at the Owens College on "The Habitable World: its discovery, partition, and commerce." The lecturer is Mr. H. Yule Oldham, and the opening meeting was held on April 4, under the presidency of Professor Boyd Dawkins.

THE combination of the Irish Natural History Societies inaugurating the *Irish Naturalist* has been joined by the newly instituted Cork Naturalists' Field Club, of which Professor M. M. Hartog is President. The first number of the *Naturalist* appeared in April, and the journal promises to be a useful record of progress in Ireland. There are contributions by A. G. More, on recent additions to the list of Irish birds; by Professor T. Johnson, on the sea-weeds of the west coast; by R. J. Ussher, on the crossbill in Ireland; by Professor Grenville Cole, on the Geology of County Dublin; and by Rev. W. F. Johnson, on the Coleoptera of Armagh. A few short notes and valuable reports of recent meetings of the Irish Societies conclude the number.

AT the Annual Meeting of the Yorkshire Naturalists' Union, held at Scarborough, the retiring President, Professor A. H. Green, F.R.S., read an address "On Some Moot Points in Geological Speculation." Mr. Charles P. Hobkirk, F.L.S., was elected President, and Committees of Research were appointed to consider the matter of local Boulders, Marine Zoology, Fossil Flora, Coast Erosion, Micro-Zoology and Micro-Botany, Disappearance of Native Plants, Geological Photographs, and Protection of Wild Birds' Eggs. Sectional officers were also appointed: in Vertebrate Zoology, Wm. Eagle Clarke, F.L.S.; Conchology, John W. Taylor, F.L.S.; Entomology, Geo. T. Porritt, F.L.S.; Botany, William West, F.L.S.; and Geology, J. W. Davis, F.G.S.

THE twenty-third Annual Meeting of the Norfolk and Norwich Naturalists' Society was held at the Norwich Museum on March 29. At the conclusion of his Presidential Address, Dr. Wheeler treated of the gradual extinction of many of the species of Lepidoptera known to have once inhabited the fenland. The species actually extinct he considered to be four in number—the Great Copper butterfly (*Polyommatus dispar*), the Gipsy moth (*Liparis dispar*), and two others little known save to entomologists, *Noctua subrosea* and *Phibalapteryx polygrammata*. After a few notes on the history and habits of these insects, he gave some account of his own experience of the Whittlesea Ermine (*Orgyia caenosa*), the Fen Leopard (*Macrogaster arundinis*), and other fen insects, which still linger in Wicken fen, but whose extinction may be looked upon as not far distant. Considering the causes of the

destruction of these insects, he thought that the direct action of man might in most cases be wholly disregarded. Indirectly, by draining the fens, man was no doubt responsible for the extinction of many of their peculiar denizens, but even this did not account for all, since some insects disappeared or became very rare without any striking change in the locality they inhabited. He considered that such cases were generally due to climatic causes, the insects being possibly on the extreme limit of their geographical area. In some cases the gradual drying of the fen might, by affecting the food-plant, prove fatal in the end to the larvæ feeding on it. Mr. Horace B. Woodward, of the Geological Survey, was elected President of the Society for the ensuing year.

We have received "The Caradoc Record of Bare Facts: a Systematic List of the more noteworthy Observations made by the Members of the Caradoc Field Club and Others, during the year 1891." The principal object of the publication is to collect and systematically arrange a series of definitely ascertained facts in the Natural History of the Caradoc District, for future reference. The services of several specialists have been enlisted, and the record is placed under their superintendence. Mr. W. Phillips is the referee for Botany, Mr. W. E. Beckwith for Ornithology, Mr. E. S. Cobbold for Entomology, and Rev. J. D. La Touche for Geology. Brief hints are given to observers, and the results for 1891 occupy eleven pages of tables. The authority for each identification is stated, and the names of Messrs. Harting, Howard Saunders, Etheridge, Lapworth, and Callaway appear among others. Such records of occurrences are of great interest and value, and we wish the Club success in its new undertaking. At the same time, we cannot refrain from urging again our contention of last month, that records of this kind ought not to be the ultimate aim of the Field Clubs. While the indexing is being done, there are ample opportunities for obtaining items of immediate importance in the progress of the philosophy of Biology and Geology; and we hope the Caradoc Club will be able to pay more attention to this aspect of its work in future Records.

In reference to our remarks last month on the Palæontographical Society's announcements, a correspondent writes as follows:—"Your comments on the Society's list of memoirs promised are opportune, inasmuch as the Annual Meeting is approaching. The list of memoirs in preparation is better than it used to be, but there are still ridiculous entries. The Society first 'promised' in 1862, and one of these early promises, the 'Graptolites,' by Wyville Thomson, was repeated until 1881. The promise in 1865 of 'Cretaceous Mollusca,' by Wiltshire and S. P. Woodward, was no doubt cancelled by the death of the latter in 1865, but the work was still kept in the list until 1891. The 'Purbeck and Rhætic Mollusca,' by Etheridge, were first promised in 1866, and still remain, though it must be as apparent to the executive as to the outsider that they might as well leave the list. The same may be said of 'Fossil Cycadeæ,' by Carruthers (1877), or even of 'Silurian Fishes,' by Harley (1883). The first part of the 'Crag Foraminifera' appeared in 1866, and the completion has been regularly promised ever since; while the memoirs on the Cretaceous and Liassic Foraminifera were 'in preparation' for at least 20 years until they finally dropped out of the list."

The *Jahrbuch der Hamburgischen Wissenschaftlichen Anstalten* for 1891 (vol. ix.) contains several contributions to the Zoology of Eastern Africa, largely based on the collections of Dr. Fr. Stuhlmann. Earthworms are described by W. Michaelsen; Termites, Odonata, and Neuroptera by A. Gerstäcker; and Mammals by T. Noack. The spiders of Madagascar and Nossibé are treated by H. Lenz; and there are some interesting notes on the tropical nut-bearing plants of Eastern Africa by R. Sadebeck.

There is also a noteworthy well-illustrated memoir on Throwing Weapons, by C. W. Lüders.

THE Italian Botanical Society invites the botanists of every nationality to an International Botanical Congress, to be held in Genoa from September 4th to 11th of the present year. Judging from the circular, this will form part of a great International Congress of "the Societies of Geography and of Natural Science," by which Genoa is solemnising "the fourth centenary of the greatest geographical discovery of all time—the work of her renowned citizen, the immortal Christopher Columbus." It will also be the occasion of the inauguration of the new Botanical Institute, which Mr. Thomas Hanbury has built and presented to the University of the town. Italian botanists cordially invite their colleagues of every nation to give the Congress an essentially cosmopolitan character, "for above all things it aims at strengthening, by the powerful influence of Science, the bonds of fraternity between nations." All enquiries and communications concerning the Botanical Congress should be addressed to Professor O. Penzig, Royal University, Genoa.

AFTER many failures, the horticulturists of Kew have succeeded in growing a young "double cocoa-nut palm" (*Lodoicea seychellarum*), which was placed for exhibition in the Victoria House last month. The plant is a native of the Seychelles, and very rarely seen in cultivation. The germination of the double cocoa-nut occupies nearly two years, and its attainment to maturity is very slow. Its peculiar mode of germination is a source of great difficulty in cultivation. The radicle grows down from the large heavy seed in the form of a stout tap-root, carrying with it the stem-bud or plumule enclosed in the sheath of the cotyledon. By the ultimate splitting of the latter the plumule is set free and able to ascend. If this long and slowly-growing process be injured success cannot be expected. The trunk of the adult tree may reach 100 feet in height, though scarcely a foot in diameter. The male and female flowers are borne on separate individuals. The immense fruits average 40 lbs. in weight; they contain, within a thick fibrous husk, one, two, or sometimes three large nuts, with hard and thick black shells, each divided about half-way down into two lobes. Before the discovery of the Seychelles islands, in 1743, considerable mystery attached to these nuts, which were often found floating in the Indian Ocean (hence the name "Coco de Mer"), and highly prized by the natives of the Archipelago. Rumphius, in his Herbarium Amboinense (1750), speaks of the nut as "hujus miri miraculi naturæ, quod princeps est omnium marinarum rerum, quae rarae habentur." It is not, he says, a terrestrial fruit which has happened to fall into the sea and thus become petrified, as Garcias ab Orta would persuade us, but a fruit actually growing in the sea, the tree being hidden from the human eye. He mentions some curious fables in connection with it, and says there are many more not worth the telling.

THE new part of the *Transactions of the Manchester Geological Society* contains Professor Boyd Dawkins' interesting paper on the "Further Discovery of Coal at Dover," to which we alluded last month. The Professor begins by expressing astonishment at the incredulity with which the discovery has hitherto been received in many scientific circles; but considering that he had never exhibited to any learned society the specimens on which his assertions were based, until the recent meeting in Manchester, we think there was good reason to suspend judgment.

FOR the first time since 1836 the Zoological Society's Gardens in London have no specimens of Giraffe. Owing to the closing of the Soudan none have been

imported into Europe for some time, and Dr. Sclater appeals to those who are entering Africa on the East Coast to help him to obtain some new specimens of this interesting mammal.

THIS year's meeting of the Museums' Association will be held at the Manchester Museum, under the Presidency of Professor Boyd Dawkins, F.R.S. The Address will be delivered on Tuesday, July 5; and the two following mornings will be devoted to the reading and discussion of papers.

WE regret to have to announce the retirement of the veteran Swedish zoologist, Professor Sven Lovén, Först Intendent of the Riks Museum, Stockholm. After a period of 51 years' service, the Professor is unfortunately compelled, by failing health, to sever his official connection with the Museum; but zoologists will be glad to learn that he is still able to continue scientific researches. At the present time, we understand, Professor Lovén has in the Press a memoir on the Cystidea, while he is also occupied with an investigation of the young stages of *Salenia*. The new keeper of the Zoological Collections at Stockholm will probably be Professor Theel, another distinguished authority on the Echinodermata.

OBITUARY.

SERENO WATSON.

BORN DECEMBER 1, 1826—DIED MARCH 9, 1892.

IN the death of Sereno Watson, Curator of the Herbarium of Harvard University, American Systematic Botany has sustained its greatest loss since that of Asa Gray. Mr. Watson was born at East Windsor Hill, Connecticut, December 1, 1826, and graduated from Yale College in 1847. For twenty years after graduation, he was engaged chiefly in teaching and in literary work. During this time he also took up the study of medicine at the University of New York. Although he had early manifested an interest in plants, and as a teacher had made some study of them, it was not until 1867 that he became actively engaged in botanical pursuits. In July of this year he joined as a volunteer the United States Geological Expedition under Clarence King, to explore the region of the fortieth parallel of latitude in Western America. For the first year Mr. Watson's work was largely topographical, but at the close of the year he took charge of the Botanical division. The material collected in the new territory was worked up in the Herbaria of Professors D. C. Eaton and Asa Gray; and in 1871, with Professor Eaton, Mr. Watson published the 5th volume of King's *Report of the Geological Exploration of the Fortieth Parallel*. It was at this time that Professor Gray invited Mr. Watson to become his assistant; and since then his life has been devoted to a critical study of the North American Flora, and to the care and development of the Gray Herbarium and Botanical Library of Harvard University. In appreciation of the high merit of his botanical work, the honorary degree of Doctor of Philosophy was conferred upon him by the University of Iowa, in which he had formerly been an instructor.

When it is considered that Dr. Watson's work in Systematic Botany began only about twenty-five years before his death, the number of his publications is wonderful. He has described over one thousand species of phaenogams, mostly from the Western United States and Mexico; and nearly all have stood the test of time and further exploration. Most of these species were published in a series of eighteen *Contributions to American Botany* in the *Proceedings of the American Academy of Arts and Science*, in which he has also elaborated many orders and genera, important among which are the

Liliaceæ, Chenopodiaceæ, *Lupinus*, *Potentilla*, and *Eriogonum*. His more extensive single works are *The Botany of California*, published in connection with Professor W. H. Brewer; and the *Bibliographical Index to North American Botany*, a most valuable work, covering the Polypetalous orders.

After the death of Thomas P. James, and during the feeble health of Lesquereux, Dr. Watson carried out successfully the editing of their work on North American Mosses; and two years ago, with Professor John M. Coulter, he revised Gray's *Manual of the Botany of the United States*, which had not been brought to date since 1867. By far the most important work Dr. Watson had undertaken was the completion of the *Synoptical Flora of North America*, begun by Dr. Gray. While he has not been spared to go far with this flora, yet many of his contributions will be of the greatest assistance in future work which may be done on the parts yet unpublished.

In 1885 Dr. Watson was in Guatemala, where he contracted the malarial fever of that climate, and since that time his constitution had not gained its former strength.

About the middle of December last he was attacked by the prevailing influenza, which later affected the heart, and on March 9 he died at his home in Cambridge, at the age of sixty-five years.

Dr. Watson was a man of high moral character as well as of scholarly attainments. He was of a retiring disposition, preferring the quiet of his study to social pleasures. His ability for continued application was remarkable, yet he was always glad to stop his more important work to give a word of advice or assistance to those whose privilege it was to work under his direction.

MERRITT LYNDON FERNALD.

Cambridge, Massachusetts.

WILLIAM BOWMAN.

BORN JULY 20, 1816—DIED MARCH 29, 1892.

ONE of the most distinguished pioneers in the modern development of Histology has just passed away in the person of Sir William Bowman, Bart. Born at Nantwich in 1816, and having received his early education at Birmingham, Bowman entered King's College Hospital, London, in 1837, and two years afterwards became Demonstrator of Anatomy in the Medical School. In 1840 he commenced surgical practice, and during the next decade all his leisure was devoted to the investigation of the minute structure of the mucous membranes of the alimentary tract, muscular fibres, the kidneys, and other tissues at that time imperfectly known. The more important results of these researches were published by the Royal Society, of which Bowman became a Fellow in 1841, and

from which he received one of the Royal Medals in the following year. Other results were incorporated in Bowman and Todd's "Physiological Anatomy and Physiology of Man," and in Todd's "Cyclopædia of Anatomy and Physiology." For an estimate of the value of the work, and its fundamental importance, we may refer to an article by Dr. Burdon Sanderson in *Nature* of April 14. In 1846 Bowman was elected assistant surgeon to the Royal London Ophthalmic Hospital, and within the next decade he attained to the position of surgeon in King's College Hospital. By this time he had decided to devote himself exclusively to the diseases of the eye, and henceforth his early brilliant researches were unfortunately discontinued. Bowman's work in ophthalmic surgery was characterised by the same thoroughness that had been conspicuous throughout his histological investigations; and, in recognition of his professional eminence he was created a baronet in 1884. Apart, however, from his purely scientific and surgical work, Sir William Bowman will long be remembered as a generous supporter of many of the principal medical and scientific institutions of London, and the Royal Institution especially mourns the loss of one of its most valued officers.

OBSERVATIONS AND CORRESPONDENCE.

Spring-time in the Waters.

Notes at the Brighton Aquarium.

The sudden outburst of animal life in the tanks of large marine aquaria is very remarkable at this season of the year. During the winter the environment is dark, dull, and void of life, and the fishes are languid and without lustre. A few warm sunny days pass and a great change is visible. It seems really as if there were a spring-time in the waters, for the rocks and sea-weeds now teem with myriad forms of life. Numberless specks of filmy substances adhere to the rocks, the thick glass fronts of the tanks, and the stones and sea-weeds placed in them.

The colours of the anemones brighten and deepen, and they appear to expand with greater freedom. That small, but beautiful species, the Daisy anemone (*Actinia bellis*), covers the tufa-rocks in thick profusion. The long, slender stems and swollen calices of the young forms now recall their budding namesakes of the meadows, and the more expanded crowns of the older, shorter, and thicker specimens resemble the full-blown flower. Delicate whorls of young serpulæ make their appearance, and the crimson-tipped plumes of innumerable *Sabellæ* wave from the mouths of their tubular dwellings, almost hidden in masses of flocculent confervæ, no longer of pallid hue but of a fresh green colour. Many beautiful young colonies of rufous, gray and purple compound ascidians of the cluster family (Botryllidæ) rapidly develop in their accustomed haunts. The violet species (*B. violaceus*) may be seen, as usual, growing on stones at the bottom of the tanks. One British species of this family (*Botrylloides fulgurale*) occurs at a depth of 530 fathoms between the Faröe Islands and the Scottish Coast.[1]

Sponges, like fungi, grow again and again in the same locality. For many seasons a sponge has appeared every spring time in a certain tank in the Aquarium. It develops and grows on the same rocky exposure year after year, splits into separate masses and falls away, leaving no apparent trace behind. When spring returns another sponge appears on that identical spot to go through similar processes of rapid growth, fission, and decay.

The first tank in the Brighton Aquarium, a very deep and light one, now presents a charming picture. The rocks at the sides are dotted, here and there, with filmy white juvenile Ascidia. Branches fixed to the floor are covered with compound forms. On one there is a purple star-like colony of *Botryllus*, and below that the young of a cream-coloured fluffy species of a solitary ascidian have settled down to pass their brief existence. On another, an encrusting compound form of the Didemnidæ may be recognised. Broad leaved

[1] W. A. Herdman, The Tunicata of the "Challenger Expedition," Part II., Ascideæ Compositæ, p. 53.

fucus of a golden brown colour and tangles of sea-weed rooted to the stones on which they grew formerly in the sea at "Black Rock" are coated here and there with a common species of moss-animals (*Flustra*) and cover the bottom of the tank. Among the broad leaves of fucus the graceful forms of the pipe fishes (*Syngnathus acus*) may be discerned on searching for them. In colour they so much resemble the weeds they frequent that it is difficult to see them, and it is not until we have watched one rise, with its central back fin revolving, like the screw of a steamer, and float slowly to the surface, that we can detect the presence of other specimens among the weeds. This is another instance of adaptive colouring subserving protective purposes, of which the soles, turbot and flounders—colourless as sand, or spotted as the shingle with which they cover respectively a great part of their flat bodies—afford more familiar examples.

Here also may be seen the male of the gleaming dark-banded "fifteen-spined stickleback" (*Gasterosteus spinachia*) which has the same propensity as its fresh-water relative, and binds together the fronds of weeds to form the nest in which the females of his kind deposit the lustrous blue vitreous ova he guards with so much pugnacity, driving away all approaching intruders. Now is the time for the males in general of the fish kind to put on their "wedding-garment" of bright colouring, like the birds of the air. The wrasse (*Labrus mixtus*), so aptly called the peacocks of the sea, are very resplendent.

Among the latest additions to the Brighton Aquarium may be chronicled the female sea-lion cub born in captivity on June 17th, 1891—the second event of the kind that has occurred during the history of the institution. In this instance, the mother had been acquired but a short time previously. This sea-lioness is an unusually fine and fully mature specimen of the Californian species, *Otaria californiana* (=*gillespii*). The night of her arrival she managed to clamber over a temporary wooden barrier raised in her enclosure, visited several tanks in succession, devouring the whiting and herrings kept in them, until her progress was arrested by a concrete party wall. She stands, when quite upright (as she has been well trained to do), with her hind limbs turned forward under her body, not less than five feet eight inches, and measures from the tip of her nose, with neck well extended, to the extremity of her hind flippers, when stretched backwards as in swimming, seven feet, and is probably one of the largest of her sex and species on record.

We are informed by Mr. W. Wells, Tank Superintendent at the Aquarium, that the cub measured eighteen inches at birth. We regret that it is impossible to furnish a portrait of the lively little animal. An early attempt was made to photograph, but owing to the extreme restlessness of the cub the "sitting" was a failure, and no proof could be developed. It was active, playful, and intelligent from the first, and took to the water six days after birth, whence its anxious parent occasionally removed it in her mouth, lifting it by the skin of the neck, as a cat does her kittens. The cub is now nine months old, and three feet six inches long, sleek and fat, and, when basking on the rocks, of a light mouse-colour. It is still nourished entirely by its most affectionate mother, who shows no sign of deserting it, and certainly never treated it with the parental indifference ascribed to the Pribyloff species *O. ursina* in the wild state, by Nordenskiöld in the "Voyage of the Vega." The parent of a male cub, born some years previously in the Aquarium, abandoned it after six months' care.

A young male sea-lion, possibly *Otaria stelleri*, a species found also off the coast of California, was purchased two years ago. It was about a year old at the time, docile, and very intelligent, and soon became a most finished performer. This animal, of a dark reddish-brown colour, with prominent eyes, has not yet acquired its full dentition. There are seven teeth in each upper jaw, but no signs of the prominent canines characteristic of the Patagonian sea-lion *Otaria jubata*.

The oldest captives are the sterlets (*Acipenser ruthenus*), first brought over from the Volga shortly after the Aquarium was opened nearly twenty years ago. Next in seniority is the Alligator Terrapin, from America (*Chelydra serpentina*) which has passed thirteen years of uneventful existence in congenial warm waters. Less than a foot long on arrival, it now measures between four and five feet from beak to tail, and its double ridged carapace is broad in proportion.

AGNES CRANE.

THE WRESTLER FROG OF LA PLATA.

MR. W. H. HUDSON, in his "Naturalist in La Plata," reviewed in the last number of this journal, relates his experience with a frog, which, instead of attempting to escape, sprang at and caught hold of the fingers of its pursuer. The frog was no doubt a breeding male of the common *Leptodactylus bullatus*. Our toad behaves just the same in early spring, and many males can be secured from the ponds, where they congregate for the purpose of breeding, by the simple method of holding out to them a walking stick which they will clasp in the manner witnessed by Mr. Hudson. But what is more extraordinary is to find our common frog (*Rana temporaria*) figured as the "Wrestler Frog" of La Plata, for no species of the genus *Rana*, nor any representative of the family Ranidæ, has yet been found in the Argentine Republic. As the specimen observed by Mr. Hudson was not preserved, the figure was probably executed at home from a British specimen. This reminds us of a similar want of accuracy in one of Gould's works, where an Australian bird is depicted in the act of seizing a newt.

G. A. BOULENGER.

EXALLAGE.

IN my article on Life-Zones in Lower Palæozoic Rocks, last month, I was in error in assigning a remark to Dr. Blanford concerning the desirability of a term for "time-varieties." Dr. Blanford has kindly called my attention to the account of a discussion in the Report of the International Geological Congress, held at Bologna in 1881 (Report, p. 168), in which the term "mutation" is suggested in the same sense as that in which the word "Exallage" was used in my article. I wish, therefore, to withdraw the term Exallage, though I still think the word "Exallagous" (not Exallogous) may be useful as an adjective.

J. E. MARR.

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